

TWENTIETH CENTURY
PSYCHOLOGY

TWENTIETH CENTURY PSYCHOLOGY

RECENT DEVELOPMENTS IN PSYCHOLOGY

PHILIP LAWRENCE HARRIMAN

Bucknell University,

Editor

with the assistance of

GRAYDON LAVERNE FREEMAN

Northwestern University

GEORGE W. HARTMANN

Teachers College, Columbia University

KURT LEWIN

State University of Iowa

ARTHUR H. MASLOW

Brooklyn College

CHARLES E. SKINNER

New York University



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INTRODUCTION

Many of the most important developments in contemporary psychology cannot be published until the war has been brought to a conclusion. Practically all the textbooks written during the decades between War I and War II referred to impetus given to group measurement of intelligence by the Army Alpha and Beta tests, to the development of personality inventories from the original *Personal Data Sheet* of Woodworth, and to various improvements over the *Man-to-Man Rating Scale* devised by W. D. Scott and others. Likewise, the writers of textbooks drew heavily upon the vast array of data gathered through the use of psychological techniques on the men of 1917-1918. Not only are the psychological procedures of today much better than those of a generation ago, but also a much greater amount of data is now being assembled. Furthermore, the psychologists are performing many more important functions during War II than merely to administer tests. Nearly half the membership of the American Psychological Association are at present in the armed forces or in some type of work which pertains directly to the war effort. At this time, however, a full account cannot be given, since much of the work is confidential and since the persons who are most intimately acquainted with it are too engrossed in their responsibilities to take leave for writing.

One of the most immediate effects of World War II has been the unavailability of many of the foreign psychological journals and monographs. Possibly, many of them have ceased publication for the duration. *Psychological Abstracts* contains a rapidly decreasing number of citations of research publications. Not a single one of the American psychological journals or mono-

graphs, however, has ceased publication; and there seems to be no reason to indicate that such action is contemplated. Although the publication of scientific research in psychology continues without marked diminution, a greater reduction may be expected. Some of the research articles now appearing in the journals were undoubtedly completed before the investigators became engaged in the pressing duties of the tasks imposed by the war. Nearly all the articles which have been published in the diverse fields of psychology show that, except for social psychology, the war has had little direct influence upon the character of the research. It may be expected that, owing to publication lag, the publications of the next few years will relate increasingly to the more expedient problems arising from the impact of total war.

This volume has been prepared for the general reader who desires to become acquainted with the work of psychologists. Whereas a textbook presents a unified, integrated digest of a large number of technical reports on psychological investigations, this book confronts the reader with specimens of the "raw material" out of which texts for the undergraduate are written. Scientific psychology is not the fascinating subject which Sunday-news-paper-supplement writers have led the public to believe. To paraphrase William James, it is, on the contrary, an unusually obstinate attempt to think clearly about behavior. Thus, these specimens have been chosen because they are illustrative of the scientific temper of the true psychologist. They also enable the reader to form some opinion about the types of problems which the modern psychologist investigates. Obviously, a volume of this type has been prepared with the intelligent reader in view; hence it makes no pretense at oversimplification. The writing of psychologists is seldom easy reading, nor does a single perusal suffice for a full comprehension of the writer's message.

From many journals, most of them difficult of access or unfamiliar to the general reader, have been selected a number of representative papers. Some of the papers have not hitherto been published. The volume does not pretend to include papers which are typical of each significant development in modern psychology. Each of them, however, deals with an important phase of modern

psychological investigation, and hence serves the purpose of acquainting the reader with significant areas of the science.

The Editor is deeply appreciative of the generous assistance given by some busy psychologists. A more agreeable arrangement than that offered by the Board of Editors of the Philosophical Library could not have been desired. Except for the statement that they had every reason to expect a collection of scholarly, scientifically acceptable papers, the publishers granted complete freedom to the editors.

Drs. G. L. Freeman, G. W. Hartmann, Kurt Lewin, A. H. Maslow, and C. E. Skinner served as the Board of Advisory Editors. For the organization of this volume, however, the Editor assumes full responsibility. Grateful acknowledgment is here expressed for their generous assistance. The Editor is indebted to the contributors and to the journal editors who graciously allowed permission for republication of their material.

PHILIP LAWRENCE HARRIMAN.

Bucknell University
Lewisburg, Pennsylvania

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TABLE OF CONTENTS

PART I. GENERAL AND THEORETICAL

	PAGE
Psychology's Progress and the Armchair Taboo	1
D. B. Klein, <i>The University of Texas</i>	
Nazi Science	10
David P. Boder, <i>Illinois Institute of Technology and the Psychological Museum</i>	
A Theory of Human Motivation	22
A. H. Maslow, <i>Brooklyn College</i>	
The Conceptual Focus of Some Psychological Systems	49
Egon Brunswik, <i>University of California</i>	
Theoretical and Experimental Difficulties of Modern Psychology with the Body-Mind Problem	64
Christian O. Weber, <i>Wells College</i>	
Personality and Typology	94
Isidor Chein, <i>College of the City of New York</i>	

PART II. SOCIAL PSYCHOLOGY

Identification and the Post-War World	119
Edward Chace Tolman, <i>University of California</i>	
The Concept of Social Status	128
Raymond B. Cattell, <i>Harvard University</i>	
Identification with Social and Economic Class	146
Hadley Cantril, <i>Princeton University</i>	
An Experimental Approach to the Study of Mob Behavior	153
Norman C. Meier, G. H. Mennenga, and H. J. Stoltz, <i>University of Iowa</i>	
Training in Democratic Leadership	175
Alex Bavelas and Kurt Lewin, <i>University of Iowa</i>	
The 100 Greatest Books Selected by 100 Qualified Persons	182
Daniel Starch, <i>Daniel Starch and Staff, New York City</i>	
A Case Study in the Perpetuation of Error	192
George W. Hartmann, <i>Harvard University</i>	
Patterns of Aggressive Behavior in Experimentally Created "Social Climates"	200
Kurt Lewin, Ronald Lippitt, and Ralph K. White, <i>Child Welfare Research Station, University of Iowa</i>	
The Authoritarian Character Structure	231
A. H. Maslow, <i>Brooklyn College</i>	

PART III. ANIMAL PSYCHOLOGY

The Experimental Embryology of Mind	245
Leonard Carmichael, <i>Tufts College</i>	
Ant Learning as a Problem in Comparative Psychology	276
T. C. Schneirla, <i>American Museum of Natural History and New York University</i>	
Contemporary American Animal Psychology in Perspective	306
T. C. Schneirla	

PART IV. EXPERIMENTAL PSYCHOLOGY

Interrlations Between Levels of Aspiration, Performance, and Estimates of Past Performance	319
James A. Bayton, <i>Virginia State College</i>	
Ocular Photography: A Scientific Approach to the Study of Human Behavior	346
Herman Francis Brandt, <i>Drake University</i>	
The Acoustic Characteristics of the Ear	371
Ernest Glen Wever, <i>Princeton University</i>	
The Psychophysiology of Set	387
R. C. Davis, <i>Indiana University</i>	
✓ The Physiological Conquest of Personality Structure	405
G. L. Freeman, <i>Northwestern University</i>	
Recent Developments in Conditioning	429
J. Donald Harris, <i>Medical Research Laboratory, U. S. Submarine Base, New London, Conn.</i>	

PART V. PSYCHOLOGY OF AESTHETIC EXPERIENCE

Aesthetic Experiences in Current Psychology	457
Max Schoen, <i>Carnegie Institute of Technology</i>	
The Expression of Meanings and Emotions in Music	468
Melvin Gillison Rigg, <i>Oklahoma Agricultural and Mechanical College</i>	
The Affective Character of Music	484
Christian Paul Heinlein, <i>Florida State College for Women</i>	

PART VI. ABNORMAL PSYCHOLOGY

Toward a Practical Concept of Neurosis	495
Knight Dunlap, <i>University of California</i>	
Psychopathy as a Psychological Problem	507
Robert M. Lindner, <i>United States Public Health Service</i>	
Pharmacological Shock Therapy as a Psychobiological Problem	517
George W. Kisker and George W. Knox, <i>Columbus State Hospital and Ohio State University</i>	

Treatment of a Case of Anxiety Hysteria by an Hypnotic Technique Employing Psychoanalytic Principles	534
Margaret Brenman and Merton M. Gill, <i>The Menninger Clinic</i>	
Concerning the Nature and Character of Post-hypnotic Behavior	547
Milton H. Erickson and Elizabeth Moore Erickson, <i>Eloise Hospital</i>	
The Experimental Induction of a Multiple Personality	591
Philip L. Harriman, <i>Bucknell University</i>	

PART VII. DIFFERENTIAL PSYCHOLOGY

The American Caste System and the Question of Negro Intelligence ...	607
Herman G. Canady, <i>West Virginia State College</i>	
The Effect of Mental Disorder on Intelligence	621
Annie Roe and David Shakow, <i>Worcester StSate Hospital</i>	
The Army Personnel Classification System	635
Walter V. Bingham, <i>Adjutant General's Office, War Department</i>	
The Rorschach Method and Its Significance in the Mental Hygiene Program	652
Marguerite R. Hertz, <i>Western Reserve University</i>	
The Patient and His Personality	685
M. R. Harrower-Erickson, <i>University of Wisconsin</i>	

PART VIII. CHILD PSYCHOLOGY

Trends in Child Psychology	705
Horace B. English, <i>Ohio State University</i>	
INDEX	712

PART I

GENERAL AND THEORETICAL

PSYCHOLOGY'S PROGRESS AND THE ARMCHAIR TABOO

D. B. KLEIN

The University of Texas

If we view the history of American psychology since the 1890's, when the American Psychological Association was founded, as a broad culture complex, we can detect the emergence at about that time of our enthusiasm for the totem of experimental techniques. This enthusiasm has become an established, orthodox tradition to which we all pay formal homage. We are proud of this totem. Even those of us who through indolence, administrative responsibilities, paucity of creative ideas, or lack of initiative have not made any sacrifices to this totem since our thesis was published, would hasten to repudiate any suggestion that our failure to sacrifice means loss of faith in the potency of the totem. We would re-enforce our repudiation by references to the laboratory courses we direct, the experimentally grounded theses we supervise, and our lip worship to the experimental gods as exemplified by the rigorous orthodoxy of our classroom lectures. Should the skeptic still harbor lurking doubts of our devotion to the official totem, we might even be constrained to furnish samples of the sincerity of our lip worship. Among these samples we might lay especial stress on the vehemence with which we eschew "armchair psychology." If experimentalism is our totem, armchair psychology is our taboo. Even amateur

anthropologists know that not all taboos are indicative of intelligent prohibitions. Some of them may be superstitious survivals of practices whose logical justification is entirely a matter of history. Others survive as verbal shibboleths due to the prestige of the elders of the tribe or to the kind of inertia responsible for the anachronism of cultural lags.

What kind of a taboo is our armchair taboo? Is it up-to-date or is it an anachronism? Does it still serve our science or has it outlived its usefulness? To the extent that one can do so within the constraints of a brief paper, we shall plead for emancipation from the shackles of this taboo. We shall endeavor to indicate that, far from making for the progress of psychology, continued allegiance to this taboo functions as an obstacle to the advancement of our professional work.

A few words regarding the origin of this armchair taboo might well be introduced at this point. The taboo emerged about 1895 at the time E. W. Scripture was instructor at Yale under the aegis of George Trumbull Ladd. Scripture himself coined the phrase "armchair psychology." It may prove helpful in the present context to recall what Boring (1, p. 514) has to say about Scripture in the following passage:

He was a great contrast to the theological and philosophical Ladd, coming into the laboratory with a strong conviction as to the scientific nature of psychology and its mission to work quantitatively upon the mind, ever approaching more and more nearly to the precision of measurement that obtains in physics. . . . (His) books still carry the fervor of the '90's: a *new* psychology, soon to be as accurate as physics!

With Ladd as senior professor it is not unlikely that Scripture, the young Wundtian enthusiast, intended his warning about armchair psychology to be a verbal thrust at the academic armor of his superior. At all events, it was seemingly more closely related to the theologically and metaphysically oriented psychology of Ladd than to Wundt's total system of psychology. We mention this because Wundt himself was by no means persuaded that the experimental approach was the exclusive scientific one for psychology. He, it will be recalled, was of the opinion that the complexities of the higher thought processes were not amenable to experimental prosecution. As a substitute for experiment

he sponsored the kind of methodological attack now associated with the work of the cultural anthropologist. The dreary volumes of Wundt's *Völkerpsychologie* constituted Wundt's own non-experimental contributions to his conception of psychology as science. It is to be doubted, therefore, whether Scripture ever intended his aversion to what he dubbed 'armchair psychology' to rule out all non-experimentally established data. It was intended as a slogan to win converts to the new psychology of the period and away from the empirically non-verifiable metaphysical presuppositions still operative among many of Ladd's philosophical contemporaries. Scripture (6, p. 2) himself granted that "simple observation of our minds" can at least give us "general outlines of facts." However, as many post-Scriptural psychologists seem to have interpreted the phrase, the armchair taboo was to make psychology a 100 per cent laboratory science.

At the turn of the century there were enthusiasts who envisaged the new psychology as a rigid laboratory discipline potentially entitled to the intellectual respect being accorded physics and chemistry. The beginner in psychology, in some places, was trained to avoid any references to mental life or human nature, no matter how confident he might be of their accuracy and cogency, unless he could cite some experimental basis for them. The empirical approach was thus being narrowly restricted to the experimental approach. All non-experimental data were stigmatized as armchair stuff. And this, despite the fact that much of the scientific work in established subjects like botany and zoology and astronomy was non-experimental. Darwin's field work, for example, was more empirical than experimental; but this fact did not induce his fellow-biologists to make short shrift of his observations by a cavalier reference to armchair biology.

We have long been puzzled by an anomaly in the early history of 20th Century American psychology. As a group these American founding fathers were rallying around the totem pole of laboratory psychology and proud of their loyalty to the armchair taboo. As a group they were teaching that the only way to know and understand mental life or behavior or consciousness

—the only way to achieve professional competence as a psychologist was to immure oneself in the laboratory. And yet when Cattell induced them to evaluate the relative eminence of the distinguished psychologists of the period they agreed, as we recall it, in voting William James the number one American psychologist. Years later when Tinker (8) and his associates duplicated the Cattell technique American psychologists were still ranking James in first place. What ought to be stressed, though, in the present context is that not even the most enthusiastic admirer of James could classify him as an experimentalist. A college junior completing one of our conventional year's courses in experimental psychology very likely does more laboratory work in nine months than James did in a lifetime. Nevertheless, twice in the history of American psychology ardent devotees of experimentalism acknowledged the professional superiority of William James, the armchair psychologist. This is tantamount to admitting that one can become an expert in the field of mental phenomena without exclusive pre-occupation with laboratory problems.

What made James a great psychologist? It could hardly have been his familiarity with experimental literature to which he contributed so little and for which he seems to have had only a rather nebulous and somewhat latent respect. Even this grudgingly granted incipient respect may sometimes have come very close to bored contempt. Certainly a phrase like "brass instrument psychology" suggests a somewhat negative attitude toward experimental work. His greatness, as we see it, was due to his empirically grounded insights into the richness and diversity of mental processes as they are experienced. James was an empiricist but not an experimentalist and his empiricism taught him more than experimentalism taught many of his colleagues. He was an armchair psychologist with his chair planted in the pulsating world of experience and not in the arid atmosphere of wordy metaphysical abstractions.

What we are urging is not that we have a moratorium on experimentation. We are in favor of more revealing and more significant experimental work in psychology. But in addition, we are for the frank recognition of the extent to which our science

can be enriched by armchair empiricism of the William James variety. Our students should be trained to be both experimental and empirical. To glorify experimentalism and to disparage empirical insights as "armchair stuff" is to blind them to much that is vital and relevant and stimulating and true. Consider, for the sake of illustration, the following passage from Stout's *Manual* written before Wertheimer's contribution of the phenomenon enabled us to enlarge our vocabulary of psychological abuse by calling people "atomists" or "associationists" or "reflexologists." In other words, even before the experimental work of the Gestaltists furnished us with the dictum of the priority of the whole, Stout (7, pp. 151-152) had already written:

A complex whole as characterized by the specific form of unity has attributes which do not belong to any or all of its parts; and inversely the parts may have attributes which do not belong to it. A heap of stones may be a pyramid, though no single stone is a pyramid; each stone may be round though the heap is not round. . . . A triangle is a closed figure. But its lines or angles are not closed figures. It is above all important from the psychological point of view that a whole object in its unity has a distinctive function and value as a factor in mental process, different from that of its parts. A melody yields a pleasure which is not due to its component tones considered apart from their union.

If we are enamoured of configurational teachings, then we ought to be enamoured of this pre-Gestalt teaching of Stout's. Stout was not an experimentalist, but this does not necessarily invalidate the cogency of his empirical observation. Psychology is richer because he made it.

A second example may be helpful in that it will serve to show that armchair observations of psychological value may be made by those who are not professional psychologists. Almost every recent text in social psychology devotes substantial space to the concept of social stereotypes. Britt's (2) work, which was published only this year, has almost fifty references to stereotyping. Who called our attention to the existence of this phenomenon? Was it a Titchener, a Thorndike, or a Pavlov? It was a journalist and a student of politics: Walter Lippmann. If stereotypes are the ubiquitous and pernicious phenomena we now recognize them to be, why did we presumed experts in the

field of human nature have to wait for a journalist to come along and shock us into a realization of their existence? My provisional answer is that we had trained our early social psychologists not to look for such phenomena. We endowed them with a phobia against critical observation of their own mental processes—and a social stereotype is a mental process—by excoriating such observation as unscientific introspection or worse yet, armchair psychology. Instead we indoctrinated them to seek scientific respectability by sticking to the language of chain reflexes, J-curves, synaptic resistances, and, in our more sadistic verbal moments, phrictopathic sensations.

By way of fortifying the general drift of this appeal a third example might be introduced. Almost all psychologists—even those most resolutely opposed to psychoanalysis because of its non-experimental nature—warn their pupils of the treacherous consequences of rationalization. Well, who taught us that such a process as rationalization exists? It was not a laboratory man, but Ernest Jones, the British disciple of Sigmund Freud. Why do we talk and write as if the process of rationalization were a scientific reality if we are committed to the dictum of repudiating all armchair contributions? Is it not because, once an armchair observer has called our attention to the existence of the process, we find such overwhelming confirmation in our own observation of our own thinking and in that of others that we are quietly compelled to acknowledge its existence? It becomes an empirically grounded fact of such cogency that to insist on experimental confirmation would be as fatuous and as superfluous as to refuse to believe that more men than women sing bass because nobody has proved this by laboratory tests or field investigations. Incidentally, it might also pay us to reflect on the possibility of a conflict between an empirical and a laboratory conclusion. The history of science ought to warn us against accepting the latter as invariably and necessarily more trustworthy than the former. Stumpf and Wundt, it will be remembered, had this kind of controversy, with Stumpf insisting that he, as a musician, knew what Wundt had to report on the matter of tonal distances as studied in the laboratory could not be squared with the empirically

grounded knowledge of the musical experts. It might also be well to reflect on the truism that careful empirical observation may often contribute more to a field than a carelessly executed experiment. Or, to put this differently: Good armchair psychology is preferable to sloppy experimentation.

We have already given some samples of good armchair psychology. Many others might be added. Freudian observations involving the existence of defense mechanisms, projections, displacements, and the concept of identification could be incorporated in a complete list. As has already been implied, even the most bitter anti-Freudians among us have found ourselves compelled tacitly to absorb some of the descriptive vocabulary this non-experimental school has introduced. What is more, some of the Freudian insights based on shrewd empirical observation are receiving experimental confirmation. Levy's (4) study of behavior differences in dogs as determined by the ease or difficulty of their nursing experiences during puppyhood is a case in point. So is Hunt's recently published study of hoarding behavior in the rat. In this connection it is relevant to introduce part of Hunt's final sentence in which he says: "These results tend to substantiate the psychoanalytic claim that infantile experience is an effective determinant of adult behavior" (3, p. 359).

For still one more example of what we regard as good armchair psychology Maslow's analysis of the distinction between deprivation and frustration might be mentioned. His differentiation between mere deprivation and deprivation symbolic of loss of prestige or social effectiveness is exceedingly valuable. In his own words:

Neglect of this distinction has created a great deal of unnecessary turmoil in psychoanalytic circles. An ever-recurring question is: Does sexual deprivation inevitably give rise to all or any of the many effects of frustration, *e.g.*, aggression, sublimation, etc. It is now well known that many cases are found in which celibacy has no psychopathological effects. What factor determines which shall be the result? Clinical work with non-neurotic peoples gives a clear answer that sexual deprivation becomes pathogenic in a severe sense only when it is felt by the individual to represent rejection by the opposite sex, inferiority, lack of

worth, lack of respect, or isolation. Sexual deprivation can be borne with relative ease by individuals for whom it has no such implications (5, p. 365).

This leads to our final hypothesis, that perhaps frustration as a single concept is less useful than the two concepts which cross-cut it, (1) deprivation, and (2) threat to the personality. Deprivation implies much less than is ordinarily implied by the concept of frustration; threat implies much more (5, p. 366).

Here we have a superb example of sound armchair psychology. It is reminiscent of the kind of analysis William James might have made. It suggests the type of keen observation of mental life our armchair taboo has tended to discourage. Without belittling experimental work, we might now be justified in encouraging our students to indulge in armchair reflection of a serious sort. We need no longer worry about the feelings of inferiority of our psychological forbears because of the greater precision and rigor of the natural sciences. The hazard of being regarded as metaphysical dreamers no longer constitutes a genuine threat to our academic respectability. We need not even be concerned about being stigmatized as philosophers. Far too many of our graduate students and teaching colleagues are too uninformed of technical philosophy to render this a serious consideration.

In other words, all of the factors responsible for the introduction and acceptance of the armchair taboo are no longer operative. It is stupid to continue to be handicapped by such an incubus. It is to be hoped that, once we become freed from it, there will be a facilitation of work in psychology. The more students are taught to study experience—the more they are encouraged to follow such keen psychological observers as Stout and Freud and James—the greater the likelihood of enriching our science by more such valuable insights as the priority of the Gestalt, rationalization, and social stereotypes. These and many others like them are armchair contributions smuggled in despite the taboo. By removing the taboo we can avail ourselves of the possibility of an increase in both the quality and quantity of such valuable intellectual commodities. Free trade is healthier than smuggling. Empirically grounded armchair psychology is healthier

than flights into the abstractions either of abstruse statistical speculations or hypothetical engrams. This ideal of a healthy empiricism does not involve a mutually exclusive, dichotomous choice. It is not a question of armchair psychology *versus* experimental psychology. On the contrary, it calls for the recognition of the essential compatibility of both approaches, their interdependence, and their value for the progress of psychology as a whole. There is no longer any basis for the laboratory man's phobia of the armchair. It might even enhance his efficiency if he could be induced to accept a change in his official furniture by consenting to draw up such a chair alongside his laboratory table.

REFERENCES

1. BORING, E. G. *A history of experimental psychology*. New York. The Century Co., 1929.
2. BRITT, S. H. *Social psychology of modern life*. New York: Farrar & Rinehart, 1941.
3. HUNT, J. McV. The effects of infant feeding-frustration upon adult hoarding in the albino rat. *J. abn. & soc. Psychol.*, 1941, 36, 338-360.
4. LEVY, D. M. Experiments on the sucking reflex and social behavior of dogs. *Amer. J. Orthopsychiat.*, 1934, 4, 203-224.
5. MASLOW, A. H. Deprivation threat and frustration. *Psychol. Rev.*, 1941, 48, 364-366.
6. SCRIPTURE, E. W. *The new psychology*. New York. Scribners, 1898.
7. STOUT, R. F. *A manual of psychology*. 3rd ed., London: Hinds, Noble & Eldredge, Inc., 1913.
8. TINKER, M. A., THUMA, B. D., & FARNSWORTH, P. R. The rating of psychologists, *Amer. J. Psychol.*, 1927, 38, 453-455.

NAZI SCIENCE

DAVID P. BODER

Illinois Institute of Technology and Psychological Museum

There is a fundamental aspect of Nazi anti-Semitism which places it in a class by itself: it is an onslaught without a chance for the victim to surrender. The pogrom regime of the Tsars, confronted the Jew with the sword in one hand and the "olive branch" (conversion) in the other. However, the Nazis feared to give to the Jews of Germany such an alternative. They were afraid that the "scape-goat" might escape. Unlike the Tsars, they did not need anti-Semitism as a theory for the justification of an existing order or for the defense of an economic class. Fascism is by no means "conservative." It is a movement toward the creation of a new order, a new ruling minority, armed to the teeth, which would divest capital and labor alike of any right to participate in the economic and social life of the nation beyond the needs of the ruling band. In short, it is a movement to enslave their own people.

The strategy of the Nazi party was, from the beginning, very similar to the now familiar strategy of its war-machine. Its first movement was a pincer attack upon the life and liberty of the German nation. As it is obvious now, the Nazi attacks upon the Versailles treaty, upon the Jews, upon religion, upon communism, were skilfully planned thrusts designed to destroy the liberal foundations of the Weimar constitution. Playing some of their intended victims against others, they got men like Thyssen to arrange "loans" for the Nazi party from financial circles out-

side the boundaries of Germany. But their purpose was always to "liquidate" these and all other aids whenever they should find them to be of no more use to them. In this, as we well know, they have been dastardly efficient.

Their "efficiency" goes further, however, than their employment of their own diabolical cleverness. They have made effective use also of the traditional "efficiency" of the German people. We have come to realize, all too sadly, how skilfully they have employed the traditional German "efficiency" in military tactics and in machine-shop practice. These, coupled with political legerdemain, have enabled them to engulf nearly the whole of Europe.

But the Nazi objective goes beyond mere military conquests. The Nazis seek to establish, not merely a "new" order, but a "permanent" order; and for this they feel it necessary to alter the established beliefs of their people. We are all familiar with the important part played by the "sociologist" Rosenberg in this respect. But not many are familiar with the uses to which the Nazis have put, not merely sociological and philosophical concepts, which traditionally lend themselves to various interpretations, but the less pliable laboratory sciences as well.

One of the beliefs which the Nazis felt it necessary to alter was the old Nietzschean Superman idea. It had to be altered because it smacked too much of self-improvement or paranoiac self-aggrandizement. As such, it had become sterile for Nazi purposes. "A *Zarathustra* in the bag of every soldier," was a slogan which Frau Foerster, the sister of Nietzsche, attempted to promote during the last war. But striving toward Superman came to be rather dull, good enough perhaps to justify the jilting of a pregnant sweetheart, or the looting by soldiers of a poor farmer, or other sadistic atrocities. In the new order such comparatively passive contemplation of an ideal had to be supplanted by an active struggle, not against a single harmless individual nor a great number of harmless individuals, but against a "type"—against a "type" that could be proved harmful to the whole "superior" race among men. And this is where the peculiar nature of Nazi anti-Semitism comes in.

It was not enough for the Nazis to rely upon the traditional objections to the Jew. And it would certainly not do to offer the Jew the choice between the sword and the "olive branch." It was necessary to establish him as a biologically inferior, and, therefore, harmful type who must cease to exist if the "superior" race is to flourish and achieve its destiny.

True to the scheme of Nazi treachery, this *anti-man* or *Gegentypus* was first presented in scientific journals without specific reference to the Jew. Nor was even the term *Gegentypus* used before the Nazis actually came to power. First hidden in scientific articles under the enigmatic name of the S-type, it was brought to light later, under Nazi "protection" and encouragement, under the new descriptive name, *anti-type*.

Men of science did not all flock to the Nazi bandwagon, of course,—not at once; but in time the march in that direction became pretty general. Thus, Philipp Lenard, the prominent German physicist and Nobel Prize winner (1920) when Hitler's place as Reichschancellor was practically assured, writes in November 1932:

"What is it that attracts me so to Hitler, who himself is no scientist? It is the respect for the truth which I always have observed in him, his wish to embrace the truth and bring it to power. . . . Who would not desire to have Hitler as leader, he who is full of respect for Truth, and fights for it; he who brings enlightenment to the millions, in the spirit of Truth?"

"The mainsprings of the National Socialist Movement which is transforming today the whole picture of reality, lie outside the boundaries of Reason," states Jaensch, the famous Marburg professor. And again in another place: "The commandment in those times [the early days of the Nazi struggle: D.P.B.] was: divided we march but united we strike. The fighter with the fist [notice *fist*, not sword: D.P.B.] had to follow his own path, and the fighter with the brain had to tread also his own—both well conscious that they belong inseparably together, that they strive toward the same goal but by different means, and that one day their forces will join and march forward together in the common struggle." So speaks Erik Jaensch, the scientific pro-

tagonist of Nazism. He sympathetically admits that among the true friends of "the great movement" are people who consider that reason [der Verstand] is to be divested of respect and even banished. He respectfully undertakes to show that "although the [Nazi] movement is applying its levers of transformation to the deeper strata of the human organism outside the boundaries of reason . . . it [the Nazi movement] does not contradict but is, on the contrary, in perfect harmony with the laws of Reason."

THE CASE OF JAENSCH

Erik Jaensch was born in 1883, and died in 1940, as professor of philosophy at the University of Marburg and President of the German Association of Psychology, a post he held since 1936. His first "election" to the executive board of the society coincides with the "election" of Adolph Hitler to the highest post of the land.

The only reason why Jaensch should be singled out for analytical study is the fact that long before 1933 he justly enjoyed world fame as an outstanding experimental psychologist. Together with his brother, Walter Jaensch, he had exhaustively studied the phenomenon of Eidetic imagery,¹ and had directed a wide group of students in the investigation of visual perception in general. However, already in the twenties, he embarked upon the field of personality studies based not directly on the facts of laboratory investigation, but rather on bizarre analogies and far-fetched inferences from laboratory findings possibly otherwise correct.

Erik Jaensch is the founder of the so-called Marburg school of psychology. It so happened that he succeeded (nominally as professor of philosophy) another notable in the history of German thought, namely, the founder of the famous Marburg school of philosophy, or Neo-Kantianism: professor Hermann Cohen (1842-1918).

Hermann Cohen became instructor at Marburg in 1873 and

¹ For a discussion on Eidetic imagery, see Kluver's article in the 14th edition of the *Encyclopedia Britannica*.

full professor in 1876. After his retirement in 1912, Cohen joined the Institute of Jewish Studies of Berlin, devoting considerable time to the study of the philosophical aspects of the Jewish religion.

According to Jaensch, Cohen (possibly in a jesting manner) called himself, during his last years of life, a Baal-Tshuvah Jew, to be interpreted as something like a prodigal son who after long years of wandering had returned to the fold. It seems to transpire from Jaensch's writings that, in spite of the fact that the onset of Cohen's career coincides with the dawn of experimental psychology, which soon spread all over the world, Cohen was unable to recognize the promise of this new scientific approach to the study of man, and seemed to have exercised somewhat more than a pure scientific opposition to the development of experimental psychology at Marburg.

We return now to Jaensch. We find in his pro-Nazi activities something more than the parading of a scientist in favor of a doctrine outside the boundaries of his own field. . . . John Dewey does it. Einstein does it. Compton does it. And so do a host of others. A scientist should be entitled to his personal views on such matters, as any other citizen would be. But the case of Jaensch is entirely different. He does not simply claim the right of the professor to his social and political convictions, but he asserts that his laboratory experiments performed under the wings of a well accredited old university, in association with a brother trained in medicine, and with numerous competent co-workers, present direct and undisputable evidence for the existence of a *biological* type with tissues and plasma inherently so constructed that he is bound, even without any intention, to exercise a disintegrating (lytic), polluting influence upon an environment which tolerates his presence.

Before continuing the discussion, I wish to state that it is not my intention to drag the lay-reader into a controversy on psychological methods. It is my opinion that the whole laboratory material of Jaensch should be made a topic of thorough statistical and experimental scrutiny. At present I am not taking issue with the experiments, but with the unwarranted inferences

and conclusions. Most of all, it is my purpose to indicate the true nature of "Nazi science."

In order not to distract the attention of the reader with constant bibliographical references, we may state here that the further discussion is based on Jaensch's latest and most extensive opus *Der Gegentypus* (the Anti-type), a volume in excess of 500 pages published in Leipzig in 1938, and to a less extent upon his other numerous writings, especially the one on "The Reconstruction of the German Student Youth and the Universities" (Leipzig 1937).

THE LABORATORY FINDINGS

A number of experiments on visual perception and the association of ideas led Jaensch, back in the twenties, to postulate the existence in the general population of two reaction types: the I-type (later renamed the J-type) and the S-type (later renamed the Anti-type). "I" stands for integration; "S" stands for synesthesia. In the words of Jaensch, "I" is the type of unified consistent personality. "S," loosely speaking, represents the opposite. So far, psychologists have barely reached any method for classification of personality types of the normal population, but I have decided for the purpose of this article to accept the factual *laboratory* material of Jaensch as correct.

Synesthesia is the tendency of some individuals to have color experiences when listening to a tone, or to music in general, and to have tone experiences when looking at colors or pictures. It has been suspected that the tendency of some painters to call their canvases by musical names, such as "Nocturne" by Whistler, or the tendency of composers to refer now and then, in their titles, to color, such as the American favorite, "The Rhapsody in Blue" by Gershwin, has its roots in synesthetic tendencies.

According to Jaensch, synesthesia is a frequent characteristic of individuals who in other ways manifest degenerative symptoms. Synesthesia is a stigma, and S means a synesthetic, stigmatized, sthenic type. (Sthenic is used not as opposite of asthenic, but as indicative of violent, intensive morbidity, or a violent de-

structive tendency toward the world.) The S-type may not necessarily "suffer" from synesthesia in the narrow sense. (The number of such cases would be rare indeed.) To Jaensch "synesthesia" means that a given content in one sensory field is associated with an experience in the other sensory field in spite of the absence of adequate stimulation. The terms are broadened to the extent of becoming meaningless.

Let us take another experiment from Jaensch's abundant arsenal, the so-called spiral experiment.



Jaensch claims that his subjects demonstrated consistently reliable differences in their reaction to the so-called spiral experiment. If the disk (see figure) is slowly rotated so that it appears to contract toward the center, and is then abruptly stopped, it creates an illusion that this disk expands eccentrically. Both the J- as well as the S-type experience the phenomenon, sometimes even with an added illusion of perspective. But for the J-type, the experience is supposedly limited to these two illusions. Not so in the case of the S-, or anti-type. The anti-type reads into the stimulus a number of fantastic components. Sixteen out of twenty-five subjects (and there is no indication that any one of them was Jewish) projected into the spiral perfectly extraneous occurrences, such as scenes from a motion picture recently seen; a few even experienced something like being caught in a storm.

No psychologist will doubt that such differences in response to a stimulus may occur. But we certainly must demand actual, statistically reliable data for tuberculosis and schizophrenic patients in juxtaposition with a normal control group before admitting (as Jaensch claims) the coincidence of certain behavior patterns with these forms of pathology. As to the specific racial correlations, the scientific requirements are as simple as they are imperative. A representative, large Jewish group and a non-Jewish group, paired for social status, intelligence, occupation, and other factors, should be submitted to the battery of Jaensch's tests. Only if statistically reliable differences can be found between the reactions of these two groups, can the racial aspect of such reactions be considered. As to the correlations of such typological characteristics with liberalism, or with an anti-Nazi outlook of life, Jaensch certainly had good opportunity to test a reliable number of Storm Troopers, on the one side, and the Jewish, as well as the non-Jewish, population of the concentration camps. But nowhere in the publications of Jaensch or his followers do we find any attempt to make use of such material.

To summarize the discussion so far: on bases of possibly reliable experiments, as such, is built a thoroughly unfounded, speculative superstructure of race theory and psychological anthropology.

Jaensch claims, on the basis of "painstaking research," to have proved "scientifically" the following postulates:

1. The population of Germany may be subdivided into two broad biological types: (1) the integrated constructive type, the J-type; (2) the disintegrating and *disintegrator* type, the S-type or Antitype (often also called the *lytic* type). Of the Antitype Jaensch writes: "Although Jewry is the main representative of the Antitype and embodies him in a large compact mass (which is of course the cause of the danger) it would be erroneous and superficial to assume that such a fundamental form of humanity is to be found among the Jews alone." In other words, the S-type, a biologically inferior type of organism, is represented by all the Jews and by numerous Germans as well. According to Jaensch the culture of Germany has been, for the centuries preceding the advent of the Nazis, a sick culture. But, says Jaensch: "Cultural politics and cultural philosophy are not bound by the picture of the average human being. They strive toward ideal norms of health, and from this stand-

point the average population of an epoch may be branded as morbid." This morbidity is, according to Jaensch, by no means allegorical. It is a biological morbidity due to organic causes, and due to a culture which for centuries has cultivated the survival of a generation of Germans polluted by heterogenous mixture of races, by tuberculosis and schizophrenia.

2. The Jew represents the antitype in pure form. Unlike the Aryan race, which is simply polluted by antitypical persons of Aryan origin, the Jewish people is *almost* entirely composed of antitypical individuals.

3. The antitype is liberal because of his biological instability. Instability in perceptual processes is linked to instability in principles of ethics and logic.

4. In the laboratory the antitype, according to Jaensch, shows the absence of a stable time sense, and the deviation of his time appreciation from actual time is large. From here on it is claimed that the J_s -type, i.e., the otherwise most integrated ideal Nordic type, behaves as if he possesses an "inner clock" which is not influenced by the content of time. Not so with the S-type. For him the subjective judgment of a time interval is greatly influenced by the content. Such an S-type supposedly talks in Bergsonian terms without ever having read anything by the famous French-Jewish thinker. "The well-defined S_1 -type shows, in the realm of time experience, the same absence of connection with objective reality which is so characteristic of the biologically determined liberalism." (p. 293). Such people are Einstein enthusiasts, without knowing much about the theory of relativity, simply because they believe that the objective and therefore compulsory order of time is being "relativised." "This is a significant characteristic of the liberalistic time experience of the disintegrator-type, as well as an indication of the cause for the excessive popularity of the theory of relativity in a culture of disintegration. The time conception of Marcel Proust *who apparently* [italics mine: D.P.B.] suffered from tuberculosis, is that of an S_1 -type, like Bergson. And Thomas Mann, in his 'Magic Mountain,' a work so eagerly read by our [apparently German. D.P.B.] tuberculosis patients, deals in a special chapter with the peculiarities of time experience of these patients."

5. The Anti-type is erratically unstable. He is subject to distraction in an unpredictable fashion.

6. The general attitudes, as well as the more complex behavior patterns of the S-type (which as we have learned includes the whole of Jewry as well as a large percentage of German Aryans) are afflicted with the following *biologically* deep-seated tendencies:

- a. Mockery and petty criticism.
- b. An unconscious tendency to caricatural misrepresentation.
- c. Artistic tendencies and theatrical abilities aimed at the schematisation of events and the ridiculing of their fellow men. Their art is expressionistic.

d. Instability of the self and the tendency to live in various roles. They are not socially inclined. The S_2 -type possesses a secondary social spirit, based on pure rational grounds.

e. Religion. The religion of the Anti-type is polytheistic, demoniacal, fetishistic, never really monotheistic. Among the extreme type, *i.e.*, the S_2 , we find dogmatism, and a religion which is reduced to methods of life. The former is a purely theoretical dogmatic system, the latter a code of life and even economics. Both are not religion in the genuine sense.

f. Social Behavior. The lytic, *i.e.*, the extreme S-type, is anti-social. Among the young of this type, there is never a true friend nor a good playmate. Disintegration breaks down the whole. The disintegrating type cannot incorporate himself into the whole of the community.

g. Diminished virility. The lytic type is always of reduced virility, he acts feminine. That is why the S-type is also the anti-type (unfit) for the army. Among males it is definitely due to the mixture of race, and to tuberculosis. Pure rational thinking and action, rigidity in purposeful behavior may simulate masculinity. But upon deeper scrutiny one discovers the femininity and the weakness of such individuals.

Space does not permit me to list all the indictments presented against a large number of the German people and against Jewry as a whole. We shall take the two indictments which we suspect may easily overwhelm many a psychologically untrained individual. These two points are: the testing of intelligence, and vocational selection.

In many a group of our society the problem of intelligence testing represents a keg of dynamite. I have no intention to present here any apologies for this branch of psychology. The accusation brought forward by Jaensch is this: The intelligence tests are constructed by individuals of the Anti-type, Jews and non-Jews, in such a manner that the Anti-type when tested excels the Nordic J-type in test performance. This unconsciously, as well as intentionally, has led to a selection for scholastic and industrial, political, and artistic leadership of Jews, and *Jew-like S-type* individuals. This accusation is thrown directly at the famous German-Jewish psychologist Stern and his many co-workers, Jews and Gentiles alike, and spreads over the whole legion of authors and users of mental tests all over the world.

A similar indictment is thrown at vocational selection, and

industrial psychology in general. The tests, according to Jaensch, are devised by authors belonging to the anti-type, and favor the selection of anti-type individuals. The emphasis in industrial testing upon quick adaptability and ability to learn quickly favors the anti-type, since he, owing to his biological instability, shows greater readiness to shift attention, and to modify his behavior. This tendency toward modifiability makes him also a better and quicker learner. But these types according to Jaensch, are also the disintegrators of culture and society; the penetration into industry of these "possibly racial hybrids, latent carriers of tuberculosis, and offsprings of the large cities," together with the undeserved rejection of the J-type on account of his poorer test performance, has caused "great damage" to German industrial life.

If we consider that most American school systems, many courts, numerous industries, and civil service commissions, and possibly the army, are using psychological tests, often very similar in principle to those used in pre-Hitler Germany, i.e. tests built on the work of Galton, Cattell, Binet, Stern, Terman, and Thorndike, we may conceive the breadth of the indictment, and its possible impact if used by a malicious demagogue.

CONCLUSIONS

Using a set of results of possibly *bona fide* psychological laboratory experiments, a presumably reputable scientist builds a superstructure of inferences to prove:

(1) That the Jews as a race, as well as a large number of Germans, are biologically unfit for a life compatible with the Destiny of the German people.

(2) That German Culture for the last three centuries has been polluted by population groups who developed an irreparable biological inferiority due to heterogeneous mixtures of races (not exclusively with Jews), which facilitates the spread of tuberculosis and schizophrenia.

(3) That these defects did not impair, but rather sharpened, the peculiar intelligence of the members of these groups, whom he finds justified in branding the anti-type. Moreover, because of their organic instability of ideals and general adaptability, persons of the anti-type have achieved political, economic, and scientific leadership.

(4) That the Jews were not the cause of the appearance of such an anti-type, but rather that the gradual development of anti-type structure

and mentality among the German tribes has led to the ready acceptance of the Jews as equals, which in turn led to the gradual fall of the German people under the leadership of the Jews and Jew-like anti-types of Nordic origin.

(5) That German philosophy was, so to speak, talmudized and that German universities have fostered the anti-type mode of thinking in all fields of learning. The great success of the theory of relativity is fundamentally a product of such a mode of thinking and is characteristic of the disintegrating type.

(6) That there is little chance to change the S-type into an integrated J-type. There is possibly some hope for the young with mild S-tendencies to be fitted into a J-like mode of behavior. But in general the S-type, that is, the Jewish people as a whole, or any S-type found in admittedly large numbers among the Aryans, "at least in the German Fatherland," must be removed from German public and economic life. For this purpose all means are justified, even the "*borrowing*" of *undesirable and cruel methods from the Antitype*.

We repeat our thesis postulated at the beginning of this article: the specific aspects of Nazi anti-Semitism lay in the definite design toward seizure of national and possibly world power by an armed clique which possesses no other positive economic or social program than the subversion of science, art, and religion and the brow-beating into submission of capital, labor and farm-folk alike.

To take lightly the thousands of pages of *Die Zeitschrift fuer Angewandte Psychologie*, for the period 1934-40, when it was dominated by Jaensch and the Marburg school in general, is to repeat the irreparable error of those who merely frowned at the Protocols of Zion some forty years ago and at Hitler's "Mein Kampf" when it first appeared.

What to do about it could probably be best decided by a committee of psychologists, anthropologists, and specialists in propaganda.

But we should never forget that, by having lent to the Fascist-Nazi movement the cloak of scientific respectability, Jaensch and his followers have created a powerful tool of popular confusion, a weapon of murderous possibilities in the hands of pseudoscientists and political charlatans in times of national crisis and stress.

A THEORY OF HUMAN MOTIVATION

A. H. MASLOW

Brooklyn College

I. INTRODUCTION

In a previous paper (13) various propositions were presented which would have to be included in any theory of human motivation that could lay claim to being definitive. These conclusions may be briefly summarized as follows:

1. The integrated wholeness of the organism must be one of the foundation stones of motivation theory.
2. The hunger drive (or any other physiological drive) was rejected as a centering point or model for a definitive theory of motivation. Any drive that is somatically based and localizable was shown to be atypical rather than typical in human motivation.
3. Such a theory should stress and center itself upon ultimate or basic goals rather than partial or superficial ones, upon ends rather than means to these ends. Such a stress would imply a more central place for unconscious than for conscious motivations.
4. There are usually available various cultural paths to the same goal. Therefore conscious, specific, local-cultural desires are not as fundamental in motivation theory as the more basic, unconscious goals.
5. Any motivated behavior, either preparatory or consummatory, must be understood to be a channel through which many basic needs may be simultaneously expressed or satisfied. Typically an act has *more* than one motivation.
6. Practically all organic states are to be understood as motivated and as motivating.
7. Human needs arrange themselves in hierarchies of prepotency. That is to say, the appearance of one need usually rests on the prior satisfaction of another, more pre-potent need. Man is a perpetually

wanting animal. Also no need or drive can be treated as if it were isolated or discrete; every drive is related to the state of satisfaction or dissatisfaction of other drives.

8. *Lists* of drives will get us nowhere for various theoretical and practical reasons. Furthermore any classification of motivations must deal with the problem of levels of specificity or generalization of the motives to be classified.

9. Classifications of motivations must be based upon goals rather than upon instigating drives or motivated behavior.

10. Motivation theory should be human-centered rather than animal-centered.

11. The situation or the field in which the organism reacts must be taken into account but the field alone can rarely serve as an exclusive explanation for behavior. Furthermore the field itself must be interpreted in terms of the organism. Field theory cannot be a substitute for motivation theory.

12. Not only the integration of the organism must be taken into account, but also the possibility of isolated, specific, partial or segmental reactions.

It has since become necessary to add to these another affirmation:

13. Motivation theory is not synonymous with behavior theory. The motivations are only one class of determinants of behavior. While behavior is almost always motivated, it is also almost always biologically, culturally and situationally determined as well.

The present paper is an attempt to formulate a positive theory of motivation which will satisfy these theoretical demands and at the same time conform to the known facts, clinical and observational as well as experimental. It derives most directly, however, from clinical experience. This theory is, I think, in the functionalist tradition of James and Dewey, and is fused with the holism of Wertheimer (19), Goldstein (6), and Gestalt Psychology, and with the dynamicism of Freud (4) and Adler (1). This fusion or synthesis may arbitrarily be called a 'general-dynamic' theory.

It is far easier to perceive and to criticize the aspects in motivation theory than to remedy them. Mostly this is because of the very serious lack of sound data in this area. I conceive this lack of sound facts to be due primarily to the absence of a valid theory of motivation. The present theory then must be considered to be a suggested program or frame-work for future

research and must stand or fall, not so much on facts available or evidence presented, as upon researches yet to be done, researches suggested perhaps, by the questions raised in this paper.

II. THE BASIC NEEDS

The 'physiological' needs.—The needs that are usually taken as the starting point for motivation theory are the so-called physiological drives. Two recent lines of research make it necessary to revise our customary notions about these needs: first, the development of the concept of homeostasis, and second, the finding that appetites (preferential choices among foods) are a fairly efficient indication of actual needs or lacks in the body.

Homeostasis refers to the body's automatic efforts to maintain a constant, normal state of the blood stream. Cannon (2) has described this process for (1) the water content of the blood, (2) salt content, (3) sugar content, (4) protein content, (5) fat content, (6) calcium content, (7) oxygen content, (8) constant hydrogen-ion level (acid-base balance) and (9) constant temperature of the blood. Obviously this list can be extended to include other minerals, the hormones, vitamins, etc.

Young in a recent article (21) has summarized the work on appetite in its relation to body needs. If the body lacks some chemical, the individual will tend to develop a specific appetite or partial hunger for that food element.

Thus it seems impossible as well as useless to make any list of fundamental physiological needs, for they can come to almost any number one might wish, depending on the degree of specificity of description. We can not identify all physiological needs as homeostatic. That sexual desire, sleepiness, sheer activity and maternal behavior in animals, are homeostatic, has not yet been demonstrated. Furthermore, this list would not include the various sensory pleasures (tastes, smells, tickling, stroking) which are probably physiological and which may become the goals of motivated behavior.

In a previous paper (13) it has been pointed out that these physiological drives or needs are to be considered unusual rather

human typical because they are isolable, and because they are localizable somatically. That is to say, they are relatively independent of each other, of other motivations and of the organism as a whole, and secondly, in many cases, it is possible to demonstrate a localized, underlying somatic base for the drive. This is true less generally than has been thought (exceptions are fatigue, sleepiness, maternal responses) but it is still true in the classic instances of hunger, sex, and thirst.

It should be pointed out again that any of the physiological needs and the consummatory behavior involved with them serve as channels for all sorts of other needs as well. That is to say, the person who thinks he is hungry may actually be seeking more for comfort, or dependence, than for vitamins or proteins. Conversely, it is possible to satisfy the hunger need in part by other activities such as drinking water or smoking cigarettes. In other words, relatively isolable as these physiological needs are, they are not completely so.

Undoubtedly these physiological needs are the most prepotent of all needs. What this means specifically is, that in the human being who is missing everything in life in an extreme fashion, it is most likely that the major motivation would be the physiological needs rather than any others. A person who is lacking food, safety, love, and esteem would most probably hunger for food more strongly than for anything else.

If all the needs are unsatisfied, and the organism is then dominated by the physiological needs, all other needs may become simply non-existent or be pushed into the background. It is then fair to characterize the whole organism by saying simply that it is hungry, for consciousness is almost completely pre-empted by hunger. All capacities are put into the service of hunger-satisfaction, and the organization of these capacities is almost entirely determined by the one purpose of satisfying hunger. The receptors and effectors, the intelligence, memory, habits, all may now be defined simply as hunger-gratifying tools. Capacities that are not useful for this purpose lie dormant, or are pushed into the background. The urge to write poetry, the desire to acquire an automobile, the interest in American history,

the desire for a new pair of shoes are, in the extreme case, forgotten or become of secondary importance. For the man who is extremely and dangerously hungry, no other interests exist but food. He dreams food, he remembers food, he thinks about food, he "emotes" only about food, he perceives only food and he wants only food. The more subtle determinants that ordinarily fuse with the physiological drives in organizing even feeding, drinking or sexual behavior, may now be so completely overwhelmed as to allow us to speak at this time (but *only* at this time) of pure hunger drive and behavior, with the one unqualified aim of relief.

Another peculiar characteristic of the human organism when it is dominated by a certain need is that the whole philosophy of the future tends also to change. For our chronically and extremely hungry man, Utopia can be defined very simply as a place where there is plenty of food. He tends to think that, if only he is guaranteed food for the rest of his life, he will be perfectly happy and will never want anything more. Life itself tends to be defined in terms of eating. Anything else will be defined as unimportant. Freedom, love, community feeling, respect, philosophy, may all be waved aside as fripperies which are useless since they fail to fill the stomach. Such a man may fairly be said to live by bread alone.

It cannot possibly be denied that such things are true but their *generality* can be denied. Emergency conditions are, almost by definition, rare in the normally functioning peaceful society. That this truism can be forgotten is due mainly to two reasons. First, rats have few motivations other than physiological ones, and since so much of the research upon motivation has been made with these animals, it is easy to carry the rat-picture over to the human being. Secondly, it is too often not realized that culture itself is an adaptive tool, one of whose main functions is to make the physiological emergencies come less and less often. In most of the known societies, chronic extreme hunger of the emergency type is rare, rather than common. In any case, this is still true in the United States. The average American citizen is experiencing appetite rather than hunger when he says "I am hungry."

He is apt to experience sheer life-and-death hunger only by accident, and then only a few times through his entire life.

Obviously, a good way to obscure the "higher" motivations, and to get a lopsided view of human capacities and human nature, is to make the organism extremely and chronically hungry or thirsty. Anyone who attempts to make an emergency picture into a typical one, and who will measure all of man's goals and desires by his behavior during extreme physiological deprivation is certainly being blind to many things. It is quite true that man lives by bread alone—when there is no bread. But what happens to man's desires when there *is* plenty of bread and when his belly is chronically filled?

At once other (and 'higher') needs emerge and these, rather than physiological hungers, dominate the organism. And when these in turn are satisfied, again new (and still 'higher') needs emerge and so on. This is what we mean by saying that the basic human needs are organized into a hierarchy of relative prepotency.

One main implication of this phrasing is that gratification becomes as important a concept as deprivation in motivation theory, for it releases the organism from the domination of a relatively more physiological need, permitting thereby the emergence of other more social goals. The physiological needs, along with their partial goals, when chronically gratified cease to exist as active determinants or organizers of behavior. They now exist only in a potential fashion in the sense that they may emerge again to dominate the organism if they are thwarted. But a want that is satisfied is no longer a want. The organism is dominated and its behavior organized only by unsatisfied needs. If hunger is satisfied, it becomes unimportant in the current dynamics of the individual.

This statement is somewhat qualified by a hypothesis to be discussed more fully later, namely that it is precisely those individuals in whom a certain need has always been satisfied who are best equipped to tolerate deprivation of that need in the future, and that furthermore, those who have been deprived in the past will react differently to current satisfactions than the one who has never been deprived.

The safety needs.—If the physiological needs are relatively well gratified, there then emerges a new set of needs, which we may categorize roughly as the safety needs. All that has been said of the physiological needs is equally true, although in lesser degree of these desires. The organism may equally well be wholly dominated by them. They may serve as the almost exclusive organizers of behavior, recruiting all the capacities of the organism in their service, and we may then fairly describe the whole organism as a safety-seeking mechanism. Again we may say of the receptors, the effectors, the intellect, and the other capacities that they are primarily safety-seeking tools. Again, as in the hungry man, we find that the dominating goal is a strong determinant not only of his current world-outlook and philosophy but also of his philosophy of the future. Practically everything looks less important than safety, (even sometimes the physiological needs which, being satisfied, are now underestimated). A man, in this state, if it is extreme enough and chronic enough, may be characterized as living almost for safety alone.

Although in this paper we are interested primarily in the needs of the adult, we can approach an understanding of his safety needs perhaps more efficiently by observation of infants and children, in whom these needs are much more simple and obvious. One reason for the clearer appearance of the threat or danger reaction in infants, is that they do not inhibit this reaction at all, whereas adults in our society have been taught to inhibit it at all costs. Thus even when adults do feel their safety to be threatened we may not be able to see this on the surface. Infants will react in a total fashion and as if they were endangered, if they are disturbed or dropped suddenly, startled by loud noises, flashing light, or other unusual sensory stimulation, by rough handling, by general loss of support in the mother's arms, or by inadequate support.¹

In infants we can also see a much more direct reaction to

¹ As the child grows up, sheer knowledge and familiarity as well as better motor development make these 'dangers' less and less dangerous and more and more manageable. Throughout life it may be said that one of the main conative functions of education is this neutralizing of apparent dangers through knowledge, e.g., I am not afraid of thunder because I know something about it.

bodily illnesses of various kinds. Sometimes these illnesses seem to be immediately and *per se* threatening and seem to make the child feel unsafe. For instance, vomiting, colic or other sharp pains seem to make the child look at the whole world in a different way. At such a moment of pain, it may be postulated that, for the child, the appearance of the whole world suddenly changes from sunniness to darkness, so to speak, and becomes a place in which anything at all might happen, in which previously stable things have suddenly become unstable. Thus a child who because of some bad food is taken ill may, for a day or two, develop fear, nightmares, and a need for protection and reassurance never seen in him before his illness.

Another indication of the child's need for safety is his preference for some kind of undisrupted routine or rhythm. He seems to want a predictable, orderly world. For instance, injustice, unfairness, or inconsistency in the parents seems to make a child feel anxious and unsafe. This attitude may be not so much because of the injustice *per se* or any particular pains involved, but rather because this treatment threatens to make the world look unreliable, or unsafe, or unpredictable. Young children seem to thrive better under a system which has at least a skeletal outline of rigidity, in which there is a schedule of a kind, some sort of routine, something that can be counted upon, not only for the present but also far into the future. Perhaps one could express this more accurately by saying that the child needs an organized world rather than an unorganized or unstructured one.

The central role of the parents and the normal family set-up are indisputable. Quarreling, physical assault, separation, divorce or death within the family may be particularly terrifying. Also parental outbursts of rage or threats of punishment directed to the child, calling his names, speaking to him harshly, shaking him, handling him roughly, or actual physical punishment sometimes elicit such total panic and terror in the child that we must assume more is involved than the physical pain alone. While it is true that in some children this terror may represent also a fear of loss of parental love, it can also occur in completely rejected

children, who seem to cling to the hating parents more for sheer safety and protection than because of hope of love.

Confronting the average child with new, unfamiliar, strange, unmanageable stimuli or situations will too frequently elicit the danger or terror reaction, as, for example, getting lost or even being separated from the parents for a short time, being confronted with new faces, new situations or new tasks, the sight of strange, unfamiliar or uncontrollable objects, illness or death. Particularly at such times, the child's frantic clinging to his parents is eloquent testimony to their role as protectors (quite apart from their roles as food-givers and love-givers).

From these and similar observations, we may generalize and say that the average child in our society generally prefers a safe, orderly, predictable, organized world, which he can count on, and in which unexpected, unmanageable or other dangerous things do not happen, and in which, in any case, he has all-powerful parents who protect and shield him from harm.

That these reactions may so easily be observed in children is in a way a proof of the fact that children in our society feel too unsafe (or, in a word, are badly brought up). Children who are reared in an unthreatening, loving family do *not* ordinarily react as we have described above (17). In such children the danger reactions are apt to come mostly to objects or situations that adults too would consider dangerous.²

The healthy, normal, fortunate adult in our culture is largely satisfied in his safety needs. The peaceful, smoothly running, 'good' society ordinarily makes its members feel safe enough from wild animals, extremes of temperature, criminals, assault and murder, tyranny, etc. Therefore, in a very real sense, he no longer has any safety needs as active motivators. Just as a stated

² A 'test battery' for safety might be confronting the child with a small exploding firecracker, or with a bewhiskered face, having the mother leave the room, putting him upon a high ladder, a hypodermic injection, having a mouse crawl up to him, etc. Of course I cannot seriously recommend the deliberate use of such 'tests' for they might very well harm the child being tested. But these and similar situations come up by the score in the child's ordinary day-to-day living and may be observed. There is no reason why these stimuli should not be used with, for example, young chimpanzees.

man no longer feels hungry, a safe man no longer feels endangered. If we wish to see these needs directly and clearly, we must turn to neurotic or near-neurotic individuals, and to the economic and social underdogs. In between these extremes, we can perceive the expressions of safety needs only in such phenomena as, for instance, the common preference for a job with tenure and protection, the desire for a savings account, and for insurance of various kinds (medical, dental, unemployment, disability, old age).

Other broader aspects of the attempt to seek safety and stability in the world are seen in the very common preference for familiar rather than unfamiliar things, or for the known rather than the unknown. The tendency to have some religion or world-philosophy that organizes the universe and the men in it into some sort of satisfactorily coherent, meaningful whole is also in part motivated by safety-seeking. Here too we may list science and philosophy in general as partially motivated by the safety needs (we shall see later that there are also other motivations to scientific, philosophical or religious endeavor).

Otherwise the need for safety is seen as an active and dominant mobilizer of the organism's resources only in emergencies, *e.g.*, war, disease, natural catastrophies, crime waves, societal disorganization, neurosis, brain injury, chronically bad situation.

Some neurotic adults in our society are, in many ways, like the unsafe child in their desire for safety, although in the former it takes on a somewhat special appearance. Their reaction is often to unknown, psychological dangers in a world that is perceived to be hostile, overwhelming and threatening. Such a person behaves as if a great catastrophe were almost always impending, *i.e.*, he is usually responding as if to an emergency. His safety needs often find specific expression in a search for a protector, or a stronger person on whom he may depend, or perhaps, a Fuehrer.

The neurotic individual may be described in a slightly different way with some usefulness as a grown-up person who retains his childish attitudes toward the world. That is to say, a neurotic adult may be said to behave 'as if' he were actually afraid

of a spanking, or of his mother's disapproval, or of being abandoned by his parents, or having his food taken away from him, It is as if his childish attitude of fear and threat reaction to a dangerous world had gone underground, and untouched by the growing up and learning processes, were now ready to be called out by any stimulus that would make a child feel endangered and threatened.³

The neurosis in which the search for safety takes its clearest form is in the compulsive-obsessive neurosis. Compulsive-obsessives try frantically to order and stabilize the world so that no unmanageable, unexpected or unfamiliar dangers will ever appear (14). They hedge themselves about with all sorts of ceremonials, rules and formulas so that every possible contingency may be provided for and so that no new contingencies may appear. They are much like the brain injured cases, described by Goldstein (6), who manage to maintain their equilibrium by avoiding everything unfamiliar and strange and by ordering their restricted world in such a neat, disciplined, orderly fashion that everything in the world can be counted upon. They try to arrange the world so that anything unexpected (dangers) cannot possibly occur. If, through no fault of their own, something unexpected does occur, they go into a panic reaction as if this unexpected occurrence constituted a grave danger. What we can see only as a none-too-strong preference in the healthy person, *e.g.*, preference for the familiar, becomes a life-and-death necessity in abnormal cases.

The love needs.—If both the physiological and the safety needs are fairly well gratified, then there will emerge the love and affection and belongingness needs, and the whole cycle already described will repeat itself with this new center. Now the person will feel keenly, as never before, the absence of friends, or a sweetheart, or a wife, or children. He will hunger for affectionate relations with people in general, namely, for a place in his group, and he will strive with great intensity to achieve his goal. He will want to attain such a place more than any-

³ Not all neurotic individuals feel unsafe. Neurosis may have at its core a thwarting of the affection and esteem needs in a person who is generally safe.

thing else in the world and may even forget that once, when he was hungry, he sneered at love.

In our society the thwarting of these needs is the most commonly found core in cases of maladjustment and more severe psychopathology. Love and affection, as well as their possible expression in sexuality, are generally looked upon with ambivalence and are customarily hedged about with many restrictions and inhibitions. Practically all theorists of psychopathology have stressed thwarting of the love needs as basic in the picture of maladjustment. Many clinical studies have therefore been made of this need and we know more about it perhaps than any of the other needs except the physiological ones (14).

One thing that must be stressed at this point is that love is not synonymous with sex. Sex may be studied as a purely physiological need. Ordinarily, sexual behavior is multi-determined, that is to say, determined not only by sexual but also by other needs, chief among which are the love and affection needs. Also not to be overlooked is the fact that the love needs involve both giving *and* receiving love.⁴

The esteem needs.—All people in our society (with a few pathological exceptions) have a need or desire for a stable, firmly based, (usually) high evaluation of themselves, for self-respect, or self-esteem, and for the esteem of others. By firmly based self-esteem, we mean that which is soundly based upon real capacity, achievement and respect from others. These needs may be classified into two subsidiary sets. These are, first, the desire for strength, for achievement, for adequacy, for confidence in the face of the world, and for independence and freedom.⁵ Secondly, we have what we may call the desire for reputation or

⁴ For further details see (12) and (16, Chap. 5).

⁵ Whether or not this particular desire is universal we do not know. The crucial question, especially important today, is "Will men who are enslaved and dominated, inevitably feel dissatisfied and rebellious?" We may assume on the basis of commonly known clinical data that a man who has known true freedom (not paid for by giving up safety and security but rather built on the basis of adequate safety and security) will not willingly or easily allow his freedom to be taken away from him. But we do not know that this is true for the person born into slavery. The events of the next decade should give us our answer. See discussion of this problem in (5).

prestige (defining it as respect or esteem from other people), recognition, attention, importance or appreciation.⁶ These needs have been relatively stressed by Alfred Adler and his followers, and have been relatively neglected by Freud and the psychoanalysts. More and more today, however, there is appearing widespread appreciation of their central importance.

Satisfaction of the self-esteem need leads to feelings of self-confidence, worth, strength, capability and adequacy of being useful and necessary in the world. But thwarting of these needs produces feelings of inferiority, of weakness and of helplessness. These feeling in turn give rise to either basic discouragement or else compensatory or neurotic trends. An appreciation of the necessity of basic self-confidence and an understanding of how helpless people are without it, can be easily gained from a study of severe traumatic neurosis (8).⁷

The need for self-actualization.—Even if all these needs are satisfied, we may still often (if not always) expect that a new discontent and restlessness will soon develop, unless the individual is doing what he is fitted for. A musician must make music, an artist must paint, a poet must write, if he is to be ultimately happy. What a man *can* be, he *must* be. This need we may call self-actualization.

This term, first coined by Kurt Goldstein, is being used in this paper in a much more specific and limited fashion. It refers to the desire for self-fulfillment, namely, to the tendency for him to become actualized in what he is potentially. This tendency might be phrased as the desire to become more and more what one is, to become everything that one is capable of becoming.

The specific form that these needs will take will of course vary greatly from person to person. In one individual it may take the form of the desire to be an ideal mother, in another it may be expressed athletically, and in still another it may be expressed in painting pictures or in inventions. It is not necessarily

⁶ Perhaps the desire for prestige and respect from others is subsidiary to the desire for self-esteem or confidence in oneself. Observation of children seems to indicate that this is so, but clinical data give no clear support for such a conclusion.

⁷ For more extensive discussion of normal self-esteem, as well as for reports of various researches, see (11).

a creative urge although in people who have any capacities for creation it will take this form.

The clear emergence of these needs rests upon prior satisfaction of the physiological, safety, love and esteem needs. We shall call people who are satisfied in these needs, basically satisfied people, and it is from these that we may expect the fullest (and healthiest) creativeness.⁸ Since, in our society, basically satisfied people are the exception, we do not know much about self-actualization, either experimentally or clinically. It remains a challenging problem for research.

The preconditions for the basic need satisfactions.—There are certain conditions which are immediate prerequisites for the basic need satisfactions. Danger to these is reacted to almost as if it were a direct danger to the basic needs themselves. Such conditions as freedom to speak, freedom to do what one wishes so long as no harm is done to others, freedom to express one's self, freedom to investigate and seek for information, freedom to defend one's self, justice, fairness, honesty, orderliness in the group are examples of such preconditions for basic need satisfactions. Thwarting in these freedoms will be reacted to with a threat or emergency response. These conditions are not ends in themselves but they are *almost* so since they are so closely related to the basic needs, which are apparently the only ends in themselves. These conditions are defended because without them the basic satisfactions are quite impossible, or at least, very severely endangered.

If we remember that the cognitive capacities (perceptual, intellectual, learning) are a set of adjustive tools, which have, among other functions, that of satisfaction of our basic needs, then it is clear that any danger to them, any deprivation or blocking of their free use, must also be indirectly threatening to the

⁸ Clearly creative behavior, like painting, is like any other behavior in having multiple determinants. It may be seen in 'innately creative' people whether they are satisfied or not, happy or unhappy, hungry or sated. Also it is clear that creative activity may be compensatory, ameliorative or purely economic. It is my impression (as yet unconfirmed) that it is possible to distinguish the artistic and intellectual products of basically satisfied people from those of basically unsatisfied people by inspection alone. In any case, here too we must distinguish, in a dynamic fashion, the overt behavior itself from its various motivations or purposes.

basic needs themselves. Such a statement is a partial solution of the general problems of curiosity, the search for knowledge, truth and wisdom, and the ever-persistent urge to solve the cosmic mysteries.

We must therefore introduce another hypothesis and speak of degrees of closeness to the basic needs, for we have already pointed out that *any* conscious desires (partial goals) are more or less important as they are more or less close to the basic needs. The same statement may be made for various behavior acts. An act is psychologically important if it contributes directly to satisfaction of basic needs. The less directly it so contributes, or the weaker this contribution is, the less important this act must be conceived to be from the point of view of dynamic psychology. A similar statement may be made for the various defense or coping mechanisms. Some are very directly related to the protection or attainment of the basic needs, others are only weakly and distantly related. Indeed, if we wished, we could speak of more basic and less basic defense mechanisms, and then affirm that danger to the more basic defenses is more threatening than danger to less basic defenses (always remembering that this is so only because of their relationship to the basic needs).

The desires to know and to understand.—So far, we have mentioned the cognitive needs only in passing. Acquiring knowledge and systematizing the universe have been considered as, in part, techniques for the achievement of basic safety in the world, or, for the intelligent man, expressions of self-actualization. Also freedom of inquiry and expression have been discussed as preconditions of satisfactions of the basic needs. True though these formulations may be, they do not constitute definitive answers to the question as to the motivation role of curiosity, learning, philosophizing, experimenting, etc. They are, at best, no more than partial answers.

This question is especially difficult because we know so little about the facts. Curiosity, exploration, desire for the facts, desire to know, may certainly be observed easily enough. The fact that they often are pursued even at great cost to the individual's safety is an earnest of the partial character of our pre-

vious discussion. In addition, the writer must admit that, though he has sufficient clinical evidence to postulate the desire to know as a very strong drive in intelligent people, no data are available for unintelligent people. It may then be largely a function of relatively high intelligence. Rather tentatively, then, and largely in the hope of stimulating discussion and research, we shall postulate a basic desire to know, to be aware of reality, to get the facts, to satisfy curiosity, or as Wertheimer phrases it, to see rather than to be blind.

This postulation, however, is not enough. Even after we know, we are impelled to know more and more minutely and microscopically on the one hand, and on the other, more and more extensively in the direction of a world philosophy, religion, etc. The facts that we require, if they are isolated or atomistic, inevitably get theorized about, and either analyzed or organized or both. This process has been phrased by some as the search for 'meaning.' We shall then postulate a desire to understand, to systematize, to organize, to analyze, to look for relations and meanings.

Once these desires are accepted for discussion, we see that they too form themselves into a small hierarchy in which the desire to know is prepotent over the desire to understand. All the characteristics of a hierarchy of prepotency that we have described above, seem to hold for this one as well.

We must guard ourselves against the too easy tendency to separate these desires from the basic needs we have discussed above, *i.e.*, to make a sharp dichotomy between 'cognitive' and 'conative' needs. The desire to know and to understand are themselves conative, *i.e.*, have a striving character, and are as much personality needs as the 'basic needs' we have already discussed (19).

III. FURTHER CHARACTERISTICS OF THE BASIC NEEDS

The degree of fixity of the hierarchy of basic needs.—We have spoken so far as if this hierarchy were a fixed order but actually it is not nearly as rigid as we may have implied. It is

true that most of the people with whom we have worked have seemed to have these basic needs in about the order that has been indicated. However, there have been a number of exceptions.

(1) There are some people in whom, for instance, self-esteem seems to be more important than love. This most common reversal in the hierarchy is usually due to the development of the notion that the person who is most likely to be loved is a strong or powerful person, one who inspires respect or fear, and who is self confident or aggressive. Therefore such people who lack love and seek it, may try hard to put on a front of aggressive, confident behavior. But essentially they seek high self-esteem and its behavior expressions more as a means-to-an-end than for its own sake; they seek self-assertion for the sake of love rather than for self-esteem itself.

(2) There are other, apparently innately creative people in whom the drive to creativeness seems to be more important than any other counter-determinant. Their creativeness might appear not as self-actualization released by basic satisfaction, but in spite of lack of basic satisfaction.

(3) In certain people the level of aspiration may be permanently deadened or lowered. That is to say, the less pre-potent goals may simply be lost, and may disappear forever, so that the person who has experienced life at a very low level, *i.e.*, chronic unemployment, may continue to be satisfied for the rest of his life if only he can get enough food.

(4) The so-called 'psychopathic personality' is another example of permanent loss of the love needs. These are people who, according to the best data available (9), have been starved for love in the earliest months of their lives and have simply lost forever the desire and the ability to give and to receive affection (as animals lose sucking or pecking reflexes that are not exercised soon enough after birth).

(5) Another cause of reversal of the hierarchy is that when a need has been satisfied for a long time, this need may be under-evaluated. People who have never experienced chronic hunger are apt to underestimate its effects and to look upon food as a rather unimportant thing. If they are dominated by a higher

need, this higher need will seem to be the most important of all. It then becomes possible, and indeed does actually happen, that they may, for the sake of this higher need, put themselves into the position of being deprived in a more basic need. We may expect that after a long-time deprivation of the more basic need there will be a tendency to re-evaluate both needs so that the more prepotent need will actually become consciously prepotent for the individual who may have given it up very lightly. Thus, a man who has given up his job rather than lose his self-respect, and who then starves for six months or so, may be willing to take his job back even at the price of losing his self-respect.

(6) Another partial explanation of *apparent* reversals is seen in the fact that we have been talking about the hierarchy of prepotency in terms of consciously felt wants or desires rather than of behavior. Looking at behavior itself may give us the wrong impression. What we have claimed is that the person will *want* the more basic of two needs when deprived in both. There is no necessary implication here that he will act upon his desires. Let us say again that there are many determinants of behavior other than the needs and desires.

(7) Perhaps more important than all these exceptions are the ones that involve ideals, high social standards, high values and the like. With such values people become martyrs; they will give up everything for the sake of a particular ideal, or value. These people may be understood, at least in part, by reference to one basic concept (or hypothesis) which may be called 'increased frustration-tolerance through early gratification.' People who have been satisfied in their basic needs throughout their lives, particularly in their earlier years, seem to develop exceptional power to withstand present or future thwarting of these needs simply because they have strong, healthy character structure as a result of basic satisfaction. They are the 'strong' people who can easily weather disagreement or opposition, who can swim against the stream of public opinion and who can stand up for the truth at great personal cost. It is just the ones who have loved and been well loved, and who have had many deep

friendships who can hold out against hatred, rejection or persecution.

I say all this in spite of the fact that there is a certain amount of sheer habituation which is also involved in any full discussion of frustration tolerance. For instance, it is likely that those persons who have been accustomed to relative starvation for a long time, are partially enabled thereby to withstand food deprivation. What sort of balance must be made between these two tendencies, of habituation on the one hand, and of past satisfaction breeding present frustration-tolerance on the other hand, remains to be worked out by further research. Meanwhile we may assume that they are both operative, side by side, since they do not contradict each other. In respect to this phenomenon of increased frustration-tolerance, it seems probable that the most important gratifications come in the first two years of life. That is to say, people who have been made secure and strong in the earliest years, tend to remain secure and strong thereafter in the face of whatever threatens.

Degrees of relative satisfaction.—So far, our theoretical discussion may have given the impression that these five sets of needs are somehow in a step-wise, all-or-none relationships to each other. We have spoken in such terms as the following: "If one need is satisfied, then another emerges." This statement might give the false impression that a need must be satisfied 100 per cent before the next need emerges. In actual fact, most members of our society who are normal, are partially satisfied in all their basic needs and partially unsatisfied in all their basic needs at the same time. A more realistic description of the hierarchy would be in terms of decreasing percentages of satisfaction as we go up the hierarchy of prepotency. For instance, if I may assign arbitrary figures for the sake of illustration, it is as if the average citizen is satisfied perhaps 85 per cent in his physiological needs, 70 per cent in his safety needs, 50 per cent in his love needs, 40 per cent in his self-esteem needs, and 10 per cent in his self-actualization needs.

As for the concept of emergence of a new need after satisfaction of the prepotent need, this emergence is not a sudden,

saltatory phenomenon but rather a gradual emergence by slow degrees from nothingness. For instance, if prepotent need A is satisfied only 10 per cent, then need B may not be visible at all. However, as this need A becomes satisfied 25 per cent, need B may emerge 5 per cent, as need A becomes satisfied 75 per cent need B may emerge 90 per cent, and so on.

Unconscious character of needs.—These needs are neither necessarily conscious nor unconscious. On the whole, however, in the average person they are more often unconscious rather than conscious. It is not necessary at this point to overhaul the tremendous mass of evidence which indicates the crucial importance of unconscious motivation. It would by now be expected, on *a priori* grounds alone, that unconscious motivations would on the whole be rather more important than the conscious motivations. What we have called the basic needs are very often largely unconscious although they may, with suitable techniques, and with sophisticated people become conscious.

Cultural specificity and generality of needs.—This classification of basic needs makes some attempt to take account of the relative unity behind the superficial differences in specific desires from one culture to another. Certainly in any particular culture an individual's conscious motivational content will usually be extremely different from the conscious motivational content of an individual in another society. However, it is the common experience of anthropologists that people, even in different societies, are much more alike than we would think from our first contact with them, and that as we know them better we seem to find more and more of this commonness. We then recognize the most startling differences to be superficial rather than basic, *e.g.*, differences in style of hairdress, clothes, tastes in food, etc. Our classification of basic needs is in part an attempt to account for this unity behind the apparent diversity from culture to culture. No claim is made that it is ultimate or universal for all cultures. The claim is made only that it is relatively *more* ultimate, more universal, more basic, than the superficial conscious desires from culture to culture, and makes a somewhat closer approach to common-human characteristics. Basic needs are *more* common-

human than superficial desires or behaviors.

Multiple motivations of behavior.—These needs must be understood *not* to be *exclusive* or single determiners of certain kinds of behavior. An example may be found in any behavior that seems to be physiologically motivated, such as eating, or sexual play or the like. The clinical psychologists have long since found that any behavior may be a channel through which flow various determinants. Or to say it in another way, most behavior is multi-motivated. Within the sphere of motivational determinants any behavior tends to be determined by several or *all* of the basic needs simultaneously rather than by only one of them. The latter would be more an exception than the former. Eating may be partially for the sake of filling the stomach, and partially for the sake of comfort and amelioration of other needs. One may make love not only for pure sexual release, but also to convince one's self of one's masculinity, or to make a conquest, to feel powerful, or to win more basic affection. As an illustration, I may point out that it would be possible (theoretically if not practically) to analyze a single act of an individual and see in it the expression of his physiological needs, his safety needs, his love needs, his esteem needs and self-actualization. This contrasts sharply with the more naive brand of trait psychology in which one trait or one motive accounts for a certain kind of act, *i.e.*, an aggressive act is traced solely to a trait of aggressiveness.

Multiple determinants of behavior.—Not all behavior is determined by the basic needs. We might even say that not all behavior is motivated. There are many determinants of behavior other than motives.⁹ For instance, one other important class of determinants is the so-called 'field' determinants. Theoretically, at least, behavior may be determined completely by the field, or even by specific isolated external stimuli, as in association of ideas, or certain conditioned reflexes. If in response to the stimulus word 'table,' I immediately perceive a memory image of a

⁹ I am aware that many psychologists and psychoanalysts use the term 'motivated' and 'determined' synonymously, *e.g.*, Freud. But I consider this an obfuscating usage. Sharp distinctions are necessary for clarity of thought, and precision in experimentation.

table, this response certainly has nothing to do with my basic needs.

Secondly, we may call attention again to the concept of 'degree of closeness to the basic needs' or 'degree of motivation.' Some behavior is highly motivated, other behavior is only weakly motivated. Some is not motivated at all (but all behavior is determined).

Another important point¹⁰ is that there is a basic difference between expressive behavior and copying behavior (functional striving, purposive goal seeking). An expressive behavior does not try to do anything; it is simply a reflection of the personality. A stupid man behaves stupidly, not because he wants to, or tries to or is motivated to, but simply because he *is* what he is. The same is true when I speak in a bass voice rather than tenor or soprano. The random movements of a healthy child, the smile on the face of a happy man even when he is alone, the springiness of the healthy man's walk, and the erectness of his carriage are other examples of expressive, non-functional behavior. Also the *style* in which a man carries out almost all his behavior, motivated as well as unmotivated, is often expressive.

We may then ask, is *all* behavior expressive or reflective of the character structure? The answer is 'No.' Rote, habitual, automatized, or conventional behavior may or may not be expressive. The same is true for most 'stimulus-bound' behaviors.

It is finally necessary to stress that expressiveness of behavior, and goal-directedness of behavior are not mutually exclusive categories. Average behavior is usually both.

Goals as centering principle in motivation theory.—It will be observed that the basic principle in our classification has been neither the instigation nor the motivated behavior but rather the functions, effects, purposes, or goals of the behavior. It has been proven sufficiently by various people that this is the most suitable point for centering in any motivation theory.¹¹

Animal- and human-centering.—This theory starts with the

¹⁰ To be discussed fully in a subsequent publication.

¹¹ The interested reader is referred to the very excellent discussion of this point in Murray's *Explorations in Personality* (15).

human being rather than any lower and presumably 'simpler' animal. Too many of the findings that have been made in animals have been proven to be true for animals but not for the human being. There is no reason whatsoever why we should start with animals in order to study human motivation. The logic or rather illogic behind this general fallacy of 'pseudo-simplicity' has been exposed often enough by philosophers and logicians as well as by scientists in each of the various fields. It is no more necessary to study animals before one can study man than it is to study mathematics before one can study geology or psychology or biology.

We may also reject the old, naive, behaviorism which assumed that it was somehow necessary, or at least more 'scientific' to judge human beings by animal standards. One consequence of this belief was that the whole notion of purpose and goal was excluded from motivational psychology simply because one could not ask a white rat about his purposes. Tolman (18) has long since proven in animal studies themselves that this exclusion was not necessary.

Motivation and the theory of psychopathogenesis.—The conscious motivational content of everyday life has, according to the foregoing, been conceived to be relatively important or unimportant accordingly as it is more or less closely related to the basic goals. A desire for an ice cream cone might actually be an indirect expression of a desire for love. If it is, then this desire for the ice cream cone becomes extremely important motivation. If however the ice cream is simply something to cool the mouth with, or a casual appetitive reaction, then the desire is relatively unimportant. Everyday conscious desires are to be regarded as symptoms, as *surface indicators of more basic needs*. If we were to take these superficial desires at their face value we would find ourselves in a state of complete confusion which could never be resolved, since we would be dealing seriously with symptoms rather than with what lay behind the symptoms.

Thwarting of unimportant desires produces no psychopathological results; thwarting of a basically important need does produce such results. Any theory of psychopathogenesis must then

be based on a sound theory of motivation. A conflict or a frustration is not necessarily pathogenic. It becomes so only when it threatens or thwarts the basic needs, or partial needs that are closely related to the basic needs (10).

The role of gratified needs.—It has been pointed out above several times that our needs usually emerge only when more prepotent needs have been gratified. Thus, gratification has an important role in motivation theory. Apart from this, however, needs cease to play an active determining or organizing role as soon as they are gratified.

What this means is that, *e.g.*, a basically satisfied person no longer has the needs for esteem, love, safety, etc. The only sense in which he might be said to have them is in the almost meta-physical sense that a sated man has hunger, or a filled bottle has emptiness. If we are interested in what *actually* motivates us, and not in what has, will, or might motivate us, then a satisfied need is not a motivator. It must be considered for all practical purposes simply not to exist, to have disappeared. This point should be emphasized because it has been either overlooked or contradicted in every theory of motivation I know.¹² The perfectly healthy, normal, fortunate man has no sex needs or hunger needs, or needs for safety, or for love, or for prestige, or self-esteem, except in stray moments of quickly passing threat. If we were to say otherwise, we should also have to aver that every man had all the pathological reflexes, *e.g.*, Babinski, etc., because if his nervous system were damaged, these would appear.

It is such considerations as these that suggest the bold postulation that a man who is thwarted in any of his basic needs may fairly be envisaged simply as a sick man. This is a fair parallel to our designation as 'sick' of the man who lacks vitamins or minerals. Who is to say that a lack of love is less important than a lack of vitamins? Since we know the pathogenic effects of love starvation, who is to say that we are invoking value-questions in an unscientific or illegitimate way, any more than the physician does who diagnoses and treats pellagra or scurvy?

¹² Note that acceptance of this theory necessitates basic revision of the Freudian theory.

If I were permitted this usage, I should then say simply that a healthy man is primarily motivated by his needs to develop and actualize his fullest potentialities and capacities. If a man has any other basic needs in any active, chronic sense, then he is simply an unhealthy man. He is as surely sick as if he had suddenly developed a strong salt-hunger or calcium hunger.¹³

If this statement seems unusual or paradoxical the reader may be assured that this is only one among many such paradoxes that will appear as we revise our ways of looking at man's deeper motivations. When we ask what man wants of life, we deal with his very essence.

IV. SUMMARY

- (1) There are at least five sets of goals, which we may call basic needs. These are briefly physiological, safety, love, esteem, and self-actualization. In addition, we are motivated by the desire to achieve or maintain the various conditions upon which these basic satisfactions rest and by certain more intellectual desires.
- (2) These basic goals are related to each other, being arranged in a hierarchy of prepotency. This means that the most prepotent goal will monopolize consciousness and will tend of itself to organize the recruitment of the various capacities of the organism. The less prepotent needs are minimized, even forgotten or denied. But when a need is fairly well satisfied, the next prepotent ('higher') need emerges, in turn to dominate the conscious life and to serve as the center of organization of behavior, since gratified needs are not active motivators.

Thus man is a perpetually wanting animal. Ordinarily the satisfaction of these wants is not altogether mutually exclusive, but only tends to be. The average member of

¹³ If we were to use the word 'sick' in this way, we should then also have to face squarely the relations of man to his society. One clear implication of our definition would be that (1) since a man is to be called sick who is basically thwarted, and (2) since such basic thwarting is made possible ultimately only by forces outside the individual, then (3) sickness in the individual must come ultimately from a sickness in the society. The 'good' or healthy society would then be defined as one that permitted man's highest purposes to emerge by satisfying all his prepotent basic needs.

our society is most often partially satisfied and partially unsatisfied in all of his wants. The hierarchy principle is usually empirically observed in terms of increasing percentages of non-satisfaction as we go up the hierarchy. Reversals of the average order of the hierarchy are sometimes observed. Also it has been observed that an individual may permanently lose the higher wants in the hierarchy under special conditions. There are not only ordinarily multiple motivations for usual behavior, but in addition many determinants other than motives.

- (3) Any thwarting or possibility of thwarting of these basic human goals, or danger or to the defenses which protect them, or to the conditions upon which they rest, is considered to be a psychological threat. With a few exceptions, all psychopathology may be partially traced to such threats. A basically thwarted man may actually be defined as a 'sick' man, if he wish.
- (4) It is such basic threats which bring about the general emergency reactions.
- (5) Certain other basic problems have not been dealt with because of limitations of space. Among these are (a) the problem of values in any definitive motivation theory, (b) the relation between appetites, desires, needs and what is 'good' for the organism, (c) the etiology of the basic needs and their possible derivation in early childhood, (d) redefinition of motivational concepts, *i.e.*, drive, desire, wish, need, goal, (e) implication of our theory for hedonistic theory, (f) the nature of the uncompleted act, of success and failure, and of aspiration-level, (g) the role of association, habit and conditioning, (h) relation to the theory of inter-personal relations, (i) implications for psychotherapy, (j) implication for theory of society, (k) the theory of selfishness, (l) the relation between needs and cultural patterns, (m) the relation between this theory and Allport's theory of functional autonomy. These as well as certain other less important questions must be considered as motivation theory attempts to become definitive.

REFERENCES

1. ADLER, A. *Social interest*, London: Faber & Faber, 1938.
2. CANNON, W. B. *Wisdom of the body*. New York: Norton, 1932.
7. HORNEY, K. *The neurotic personality of our time*. New York: Norton, 1937.
4. FREUD, S. *New introductory lectures on psychoanalysis*. New York: Norton, 1933.
5. FROMM, E. *Escape from freedom*. New York: Farrar and Rinehart, 1941.
6. GOLDSTEIN, K. *The organism*. New York: American Book Co., 1939.
7. HORNEY, K. *The neurotic personality of our time*. New York: Norton, 1937.
8. KARDINER, A. *The traumatic neuroses of war*. New York: Hoeber, 1941.
9. LEVY, D. M. Primary affect hunger. *Amer. J. Psychiat.*, 1937, 94, 643-652.
10. MASLOW, A. H. Conflict, frustration, and the theory of threat. *J. Abnorm. soc. Psychol.*, 1943, 38, 81-86.
11. ———. Dominance, personality and social behavior in women. *J. soc. Psychol.*, 1939, 10, 3-39.
12. ———. The dynamics of psychological security-insecurity. *Character & Pers.*, 1942, 10, 331-344.
13. ———. A preface to motivation theory. *Psychosomatic Med.*, 1943, 5, 85-92.
14. ———, & MITTELMANN, B. *Principles of abnormal psychology*. New York: Harper & Bros., 1941.
15. MURRAY, H. A., et al. *Explorations in personality*. New York: Oxford University Press, 1938.
16. PLANT, J. *Personality and the cultural pattern*. New York: Commonwealth Fund, 1937.
17. SHIRLEY, M. Children's adjustments to a strange situation. *J. abnorm. soc. Psychol.*, 1942, 37, 201-217.
18. TOLMAN, E. C. *Purposive behavior in animals and men*. New York: Century, 1932.
19. WERTHEIMER, M. Unpublished lectures at the New School for Social Research.
20. YOUNG, P. T. *Motivation of behavior*. New York: John Wiley & Sons, 1936.
21. ———. The experimental analysis of appetite. *Psychol. Bull.*, 1941, 38, 129-164.

THE CONCEPTUAL FOCUS OF SOME PSYCHOLOGICAL SYSTEMS

EGON BRUNSWIK

University of California

In the present paper the attempt is made to order systematically some of the conceptual tools which have been used in dealing with psychological topics. In the opinion of the author, a suitable starting point for such a consideration is furnished by a scheme of the following kind (Fig. 1).

The drawing represents an organism within its surroundings as described by an observing physicist in terms of measurement and computation. This observer might be able to distinguish different layers within the whole causal texture with reference to the organism. Some of those which became most outstanding in psychological discriminations might be designated by the terms (*c*) remote past, (*b*) the realm of palpable bodies in the actual environment, (*a*) stimulus events located on the retina or on other stimulus surfaces of the organism, (0) intraorganismic events, (*A*) muscular reactions, or behavior in the narrower sense of the word, (*B*) effects of these reactions with regard to the relationship between organism and surroundings, as *e.g.*, the reaching of a goal, and finally (*C*) the more remote consequences and final products of life activities including stabilized mechanical or conceptual tools for further use. For the purpose of further explanation, some of the customary terms not used in this list are included in the chart.

LAYERS

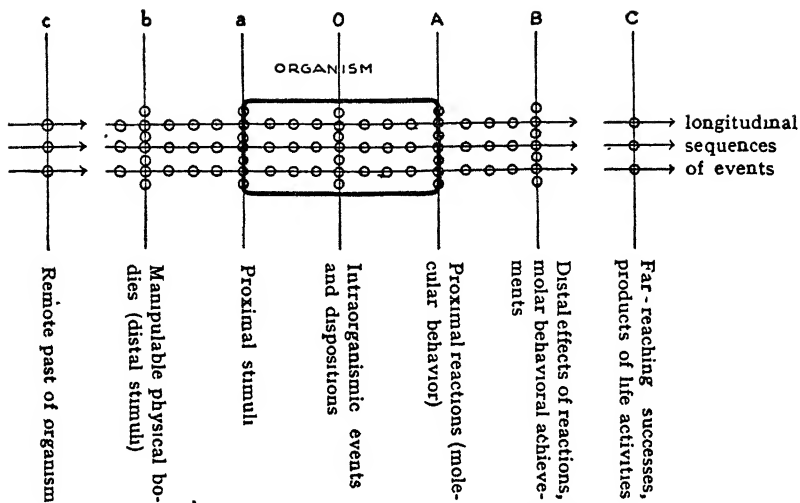


Fig. 1. Scheme of the organism in its surroundings.

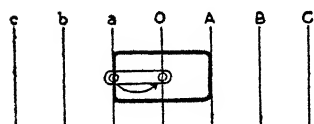


Fig. 2. Early Psychophysics

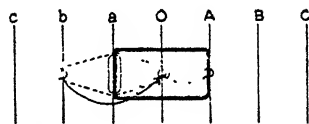


Fig. 5. Thing-constancy Research

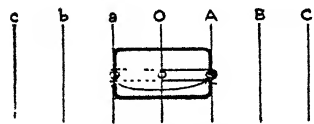


Fig. 3. Early Behaviorism

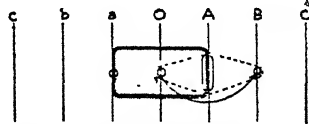


Fig. 6. Molar Behaviorism

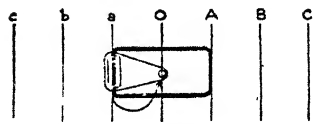


Fig. 4. Gestalt Psychology

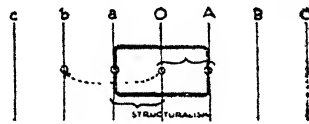


Fig. 7. Introspectionism

The layers indicated are not supposed to designate singular sequences in time, but rather to furnish a general scheme for cross-sectional classification and coordination of physical events, or features of the physical world, with reference to their causal relationship to an organism. The scheme possesses a certain symmetry, with layers designated by corresponding letters (*a* and *A*, *b* and *B*, *c* and *C*) conceptually related to each other.

Four main types of interest seem to be possible within this system: (1) emphasis upon events belonging to a certain cross-sectional layer and the internal relations of these events among each other, (2) emphasis upon a certain type of causal chains, that is interest in longitudinal sequences, (3) emphasis upon the external relationships of distant cross-sectional layers among each other, (4) emphasis upon the interrelations of discrete longitudinal patterns among each other.

Concepts and laws referring to (1) and (2), that is, to single events or to cross-sectional or longitudinal internal relationships, are non-psychological. Roughly speaking, these internal relationships constitute the core of the problems treated in physics proper as far as the left part of the picture and particularly (*b*) is concerned. They constitute the core of the biological sciences in the narrower sense of the word as far as the middle of the picture is concerned, and the "letters" ("humanities") or "Geisteswissenschaften" as far as the right part is concerned. On this latter side those physical features of the world are represented which still reveal the fact that their causal ancestry is partially built up from causal patterns typical to life activities. For example, a mechanical tool made by man would be considered to belong to this group only by virtue of its particular form, in connection with a limited number of certain "relevant" properties, regardless of all the other traits.

We are now going to attempt to characterize some of the psychological disciplines, or systems, as they grew up historically, in terms of our scheme. In every instance, not the programmatically propounded general frame will be taken as the standard, but rather the conceptual texture of the work actually performed in a sufficiently detailed fashion. This has to be done, since in

an arm-chair sense nearly any of the systems might justly consider itself all-inclusive, and able to be conciliatory in face of every objection without doing violence to its own conceptual frame work. In short, we are going to treat systems not in terms of what they could have included but what they did include.

Early experimental psychology still was characterized by the ideology typical of the most exemplary non-psychological sciences. In particular its interests were, as far as the very beginnings are concerned, chiefly longitudinal in the sense characterized above under (2). The first type of experimental research to win importance in psychology was classical psychophysics. Its interest centered around a rather limited fragment of a type of longitudinal chain *per se*. The initial link was defined in terms of what Koffka has named proximal stimulation, that is, stimulation in terms of the causal chains as they just enter the sense organ. The guiding ideal of the psychology of that time was expressed by the wish to know as much as possible about the functional mechanisms of the sense organ and of nervous conduction, — in short, about “mediation problems” —, and thus to be enabled to pursue the causal chains as closely as possible, in a step-by-step fashion, so to speak. There was, however, as we follow the development from the Johannes Müller to the Fechner era, a noticeable shift of emphasis toward the relationship, *per se*, of the two end terms of the longitudinal fragment concerned, namely, the relationship between “stimulus” and “sensation”. At the same time the problems of the causal “mediations”, as such, were losing ground. It is this becoming more and more interested in a by-and-large causal correlation between discrete layers regardless of the technicalities of their interconnection which brought psychology proper into existence as a discipline distinguishable from physiology.

Fig. 2 gives a schematic picture of early psychophysics using the scheme of Figure 1 as a frame of reference. The focus of concept-formation is located at the layers of proximal stimulation and of internal response as well as at their gross interrelation as indicated by the arrow. The interest in mediation problems still vital is represented by the slope covering the entire ground lead-

ing from the one term to the other in longitudinal direction.

The categorical structure and the actual research interest of the early "conditioned reflex" behaviorism as represented by Pavlov or by Watson is similar in principle to that of early psychophysics. The chief conceptual emphasis appears to be shifted, however, from the implicit to the overt response in terms of bodily movements as such. The interest in mediation problems is centered around the motor rather than the sensory processes, as indicated in Figure 3. — In fact, of course, every psychophysics or introspective psychology utilized verbal, that is, a particular kind of motor, responses. These responses were supposed to be, however, true representatives of inner states. The aimed-at-focus of concept formation of these disciplines might therefore be considered to lie in the internal life of the individual.

The further development of psychology as an exact science can be characterized as a progressive extension of the range of consideration from the fragmentary or molecular viewpoint to larger units of a "molar" nature. This goes with an increasing emphasis upon gross by-and-large correlations between kinds of events schematically located at a distance from each other, on the one hand, and with an — on the whole — more and more subordinate interest in mediation, *per se*.

A first important step was Gestalt psychology. Considering the most characteristic core of problems actually treated by Gestalt psychology in the field of perception, the chief difference as compared with traditional psychophysics lies in an extension of the notion of the stimulus to that of a stimulus pattern. The response is treated as a response to the sensory configuration as a whole whereby the laws of dynamic interaction within each of the cross-sectional layers of the sensorium are made the central issue. Gestalt psychology, though complex, i.e., molar, is, however, still fragmentary insofar as it is, in its most elaborate parts, a psychology "from the retina inward", so to speak. There is, as in psychophysics, a great deal of interest in mediation problems, as can be seen from the numerous attempts to explain physiologically the facts found. All this is represented schematically in Figure 4.

A further extension of the psychology of perception is given by including into the scope of consideration the manipulable solid bodies, located in the father environment, and their recognition as the specific determiners of the reaction. The beginnings of this line of interest can be traced back to Helmholtz. This trend, however, did not become conscious of its own character until the last few decades and after the earlier stages of Gestalt psychology already had been completed. In this discipline, the stimulus is not any longer defined in proximal terms but in distal ones. The actual research centers around the question to what extent the perceptual system is able to liberate itself from the disturbing variability of the proximal representation of similar distal stimuli and thus to focus the response upon the latter and not upon the former as the determining event. In other words, the question is how far the organism has established mechanisms which are able to extrapolate, with a sufficiently large chance of success, the causal chains from the retina backward and thus, figuratively speaking, to reach out cognitively into the farther surroundings.

A fairly univocal attachment of a class of reactions to distal properties, like extension of reflectivity to color, despite changes in the mediating causal pattern, is called "thing constancy". We might, then, as well describe these reactions by means of the other term of the external relation in question, or in short, "in terms of their objects attained." The focus of concept formation is thus shifted away from the organism itself into its farther surroundings, or, more precisely, into a relationship of the organism with layer (*b*). In other words, the organism is characterized by its ability to achieve something with regard to its environment, not by the intrinsic character of its reactions or by the nature of certain physiological forms of mediation.

In the opinion of the author, there is scarcely another discipline which would reveal as clearly as does constancy research the extent to which the organism is able to render irrelevant the particularities of mediation. Let us take a frequently quoted example. Among the chief constituents of the system of cues which enable the organism to extrapolate the sizes of the surrounding bodies from the retinal stimulus pattern, are the so-

called "distance-cues", as, for instance, binocular disparity or the perspective distortion of right angles. There are numerous kinds of distance cues. Most of them differ radically from each other as long as we consider intrinsic properties or the physiological mechanisms operated by them. They have in common nothing but a higher or lesser probability of being caused by a certain environmental depth-pattern. And yet they are responded to by the central system of the organism in an identical manner, or in short, they are "equipotential". It is this feature of organismic reaction and achievement which forces psychology, as it approaches its genuine molar and relational problems, more and more into a focusing upon the end terms of far-reaching relationships. The particular "how" of the mediation processes, on the other hand, necessarily will attract only subordinate interest.

In other words, we do not consider it a matter of choice, whether psychology does focus its concepts on one or on another layer or on a correlation of layers among each other. In looking without preconceptions at nature populated by organisms, gross correlations of higher or lesser degree between kinds of events rather remote from each other in space or time will strike the observer. The network of occurrences participating in such correlations might be conceptually picked out and its constituents labeled as the given foci of life patterns. Psychology has to focus its descriptions on what the organisms have become focussed on, not on events systematically located at the interstices between these foci. *Ordo idearum sit idem ac ordo rerum*. There has to be a discipline to deal with these foci and their gross correlations, *per se*. Otherwise there would remain a white spot on the landscape of possible scientific knowledge. By all of its history, it is psychology which is predestined to fill this gap.

In short, molar psychology of achievement is a deliberate "lump"-treatment. This feature seems to be the chief obstacle which stands in the way of its acceptance. Correlations between distant layers never hold to an ideal degree. There are always "exceptions" due to the lack of perfection of the cues and means establishing these correlations. In every instance, there is only

a higher or lesser degree of probability for the reaching of the usual end. This feature becomes especially clear where we have to do with the so called instincts. Dealing with instruments of this kind in terms of their achievement leads to an apparent lack of exactitude. It takes a certain courage, a neglect of some of the attitudes sacred to scientific tradition, to give up the safety of molecular correlations, cheap to obtain as they are, in favor of the equivocalities or "vaguenesses" of molar correlations. But we have to prefer vagueness focused upon essentials to security and strict univocality focused upon non-essentials. This holds especially as soon as we are lucky enough to find everything prepared to become strictly physicalistic in our "vaguenesses", quantifying them by the means of correlation statistics and other related mathematical tools.

There always remains a certain self-restriction required in order not to become too curious about the mechanisms causing the "exceptions" and dispersions mentioned, before the task of a bird's-eye-view-inventory of gross correlations has been completed. Of course, there are various ramifications. Looking for exceptions and their causes might, besides being a mere side track, become a corrective measure which enables us to find still more super-ordinate correlations. These super-ordinate correlations, indeed, should be our ultimate aim. Furthermore, concepts and methods referring to mediation problems will have to come back to psychology proper as soon as the precise limitations of the complex achievements in question are subject to closer examination. These problems are out of the scope of psychological consideration only so far as that first phase of research is concerned in which far-reaching gross achievements become discovered and examined in first approximation. On the whole, however, psychology should develop "from above", not "from below". It might proceed to sub-foci of a more and more particular kind and ultimately converge towards and merge with its complementary sciences of a genuinely molecular type.

The schematic representation of constancy research (Fig. 5) has to be drawn in the following way: an arrow from a certain type of events in (*b*) to a certain type of events in (0), repre-

senting the primary interest; a slope around the group of sub-foci which constitute the "family" of equipotential cue patterns and which circumscribe the extent of variability of the mediational pattern and thus the degree of safeguardedness of the achievement under varying conditions of mediation; and finally a slope around the whole unit of processes involved, including mediation processes. The latter slope has been dotted in order to indicate the subordinate nature of the mediation problems. A further dotted arrow is drawn to connect the event in (0) with an overt response. This is done to indicate that Psychology in Terms of Objects wishes to be, in principle, strictly behavioristic, *i.e.*, refuses to extrapolate without particular controls from the measurable verbal utterances into the field of their internal "meanings".

A picture symmetrical to that of constancy-research is yielded by a chief part of the research done within the conceptual frame of molar or purposive behaviorism, as represented by Tolman (Figure 6). The difference is merely a material one, constancy research being concerned with problems of reception and cognition, or the organismic achievement of a backward extrapolation of causal chains, whereas molar behaviorism deals with problems of overt action and its further environmental effects. In molar behaviorism, as contrasted to molecular behaviorism, results are expressed in terms of reaching a certain goal, not in terms of movements made. The comparative irrelevancy of ways and means, that is, their equipotentiality with regard to a certain end, is experimentally demonstrable. As it was done for the proximal stimulus cues in the extended psychophysics of perceptual thing constancy, molar behaviorism realizes that the essentials of behavior will become lost in a description focused on proximal determination. Thus both disciplines are essentially environmentalistic, not mediationalistic or physiologistic.

In both constancy research and molar behaviorism a certain interrelation of longitudinal causal chains is made one of the central issues, namely, their equipotentiality within the larger instrument of a well-established far-reaching causal coupling. Emphasis is withdrawn, to a certain extent, from a step-by-step determination of these mediating chains of events. Such a re-

striction is not essential, however, to a molar point of view, as is shown by the type of approach represented by Hull. His general frame of consideration coincides in its most essential features with that of the disciplines mentioned. A still stronger line of interest is focussed, however, on the "family" of mediational patterns, *per se*. These patterns are analyzed in an essentially associationistic or conditioned reflex fashion, that is, in a molecular longitudinal way. In the opinion of the author, the chief objection to such an attitude is a merely practical one, namely distraction from the gross "first-approximation" treatment of cognitive or behavioral achievement.

The idea of a pure achievement analysis is accomplished, more thoroughly than in any of the other branches mentioned, in the psychology of "tests". At first glance this might seem to be a strictly cross-sectional affair within events in layer (*B*), these events being correlated among each other statistically. The correlational analysis implies, however, the reference to organisms performing various combinations of achievement. Mediation problems are usually kept entirely outside of consideration.

It can even be said that correlation statistics as a general scientific instrument received a decisive impetus from test psychology (Pearson, Spearman, Thurstone, and others). Starting from rather complex achievements relatively detached from straight sensory or muscular activities, test psychology had the chance to grow up without meeting a resistance comparable to that met by Gestalt psychology or the other molar disciplines mentioned above. The methods developed in test statistics are, therefore, most likely to become exemplary to all future molar psychology. As an illustration it might be mentioned that, according to a recent American survey, the term correlation is among the two or three most frequently quoted terms to be found in the textbooks used in this country.

In recent times statistical analysis led to a closer reference to a small number of hypothetical "factors" or basic abilities independent of each other, underlying the countless variety of actual performances. This is one of the instances where the stage is set for a genuinely psychological physiological, focused not on

layer (0), as such, or on its interrelations with layers (*a*) or (*A*), but on the far-reaching interrelations between (0), on the one and, and (*B*) — or (*b*) —, on the other.

A few words only about disciplines like social psychology, genetic psychology, psychoanalysis: they all seem to be focused primarily on molar interrelations of the organism in its actuality, with some complex features of the remote environment, present or past. They fulfill the requirements of a molar psychology as long as they concentrate upon an attempt to segregate abstractively the focal or relevant traits within the patterns they investigate from the actually irrelevant ones.

A certain type of genetic attitude possesses, however, a close resemblance to molecularism. Considering the systematic description of gross achievement or adjustment of the organism to the environment as the primary subject matter of psychology, inquiry about the history of such mechanisms in some cases might easily lose contact with the essential features of the achievemental pattern actually in question. In such instances, asking "why" becomes comparable to the "how" problems of the mediationalistic type. For example, to be concerned primarily as to whether a certain organismic instrument is due to heredity or to learning, might occasionally become just another burden for the investigator of that instrument, coordinate with the claim of the physiologically minded criticist whose first concern is to know as much as possible about all the single steps involved in the mechanism in question. Like molecularism, geneticism for its own sake involves the danger of diverting psychology into knowing more and more for the price of knowing it about less and less, or about smaller and smaller fragments of the units which constitute the task of psychology.

In the common language of science, molecular as well as genetic descriptions have often been called "explanations". In contrast to that, molar achievemental analysis is "descriptive" in the most restricted sense of the word. As a deliberate "lump" treatment, it refuses to aim at explanation for its own sake. It is a psychology "in terms of . . .", a terminological affair, a way

of registering and conceptually looking at gross correlations in their straightforward actuality.

Up to this point of our considerations psychology has been treated as if it were built up by means of strictly scientific methods, that is, in principle, as physics of a certain group of causal correlations. For a large part of psychology this holds true, in principle at least. The events involved are subject to measurement and the interrelations to quantitative treatment. Or, as Lewin would put it, "Aristotelian" concept formation in terms of absolute dichotomies between qualitatively different "principles", as, *e.g.*, the traditional antithesis of "insight" versus "learning", has already been largely replaced by a "Galilean", that is by more "diagrammatic" forms of thinking in terms of gradual discriminations.

We do not wish, however, to conclude this paper without glancing at some of the forms of psychology which do not, or do not fully subscribe to such a methodological ideal.

First of all, there is introspectionism. Common to all introspectionism is the tacit assumption of a strict one to one relationship between verbal utterances and "inner events". Only by virtue of such an attitude is it possible to consider, as is done by introspectionism, words or other events located in layer (A) as valid representatives or "symbols" for inner experiences (0). In all objective psychology verbal utterances are taken not as symbols, but merely as "symptoms" the meaning of which is supposed to be accessible only by means of special correlational investigations. In Figure 7, the substitution mentioned is represented by a brace.

Another kind of substitution is, however, much more fundamental in introspectionism. As emphasized especially by the so-called act-psychologists, *e.g.*, Brentano, the essence of consciousness is characterized by its pointing toward, or aiming at, an object. This relationship has been called intentionality. Though it was said that intentional objects should not be confused with the physical environment, it still can be made clear that introspectionism became infiltrated with a conceptual structure taken to a large extent from the layer of palpable bodies (*b*).

Yet there was no chance of a quantitative treatment on a physicalistic basis, since the relation of (0) to (*b*) — or to something formally analogous to (*b*) — had been accepted as univocal without experimentation. Furthermore, this relationship was regarded as a qualitative entity of its own kind entirely incomparable with the causal relationship, to which it is also supposed to be opposite in direction. This relationship was admitted without further control, from a mere inspection of layer (0). This is indicated in Figure 7 by a dashed arrow $0 \rightarrow b$ which is also pointing in the opposite direction from the corresponding arrow $b \rightarrow 0$ in Figure 5. In philosophy, the problems of "dualism" have to a large extent arisen from confusion and uncritical mutual substitution of the two cross-sectional layers structurally similar to each other. This substitution is comparable to that committed by introspectionism. The fallacies of an uncontrolled substitution of layers by each other have recently been emphasized by Heider.

Introspectionism can be subdivided into two main branches. The one is represented by men like Wundt and Titchener, and also by Mach. It is sometimes called "Structuralism". Its chief feature is to look for basic elements out of which all the complex experiences may "consist" (without questioning whether the grammar of the word "consist" permits such an application). Structuralism coincides in time with the early molecular sensory psychology characterized by its emphasis upon mediational features like proximal stimulation and the structure of the sense receptors. It is obvious that in this general attitude — sometimes characterized as "glorification of the skin" — the mosaic-nature of the events at the sensory surface has been directly carried over to the hypothetical structure of inner events (cf. Fig. 7). Thus these came to be understood after the pattern of the sense organ. In structuralism, therefore, not only layers (0) and (*A*) and layers (0) and (*b*), but also layers (*J*) and (*a*) appear in uncontrolled confusion.

The second branch of introspectionism might be called phenomenism. It is somewhat related to act psychology, and sometimes the term phenomenology is applied, not quite unmis-

ably, to it. It is the kind of introspection represented by Gestalt psychology and the Würzburg school of psychology of thinking. There was sufficient sophistication within phenomenism about the naive entanglement of structuralism with sensory elementarism, with mediationalism and with functional "explanation". Unbiased "description" of the pre-analytically given was aimed at. The structuralist's "consist of" was given up in favor of the phenomenist's "resembles". Everyday language and even slang was used deliberately. Characteristic examples are the description of the phenomenon of the shadow by Hering as a tiny skin of darkness lying upon the surface of the object, the true color of which shines through the former, or the introduction of the term "Aha-Erlebnis" by Bühler in order to refer to the experience of sudden insight. In this way, phenomenism grew into a kind of conceptualized and systematized poetry, bringing, in principle, all the various concepts and terms of the common qualitative language into one comprehensive system of resemblances. Since all "qualities" might be regarded as gross reactions of the organism to some features of the environment and thus be systematically located in layer (0), phenomenism is the strictest expression in existence of an 0-internal system of psychological concept formation.

As a system of mutual resemblances, phenomenism can be represented by means of a spatial order. The best example for such a quasi-spatial arrangement of qualities, though limited to a certain modality, is the three-dimensional Hering color pyramid. It is built up on an entirely phenomenistic basis regardless of the physical relationships of colors among each other. Thus it deals with reactions only, not with stimuli. It was the first attempt to deal with psychological problems on a "topological" basis by assigning a certain place in a spatial order to each quality. These qualities could then be determined in terms of basic "dimensions" defined by certain outstanding qualities.

On a somewhat different basis, topological considerations have been recently introduced into psychology by Lewin. In his *Topological Psychology*, the actual "life space" is represented by a spatial scheme. As is true for phenomenism, however, not

the surroundings defined in terms of physics are taken as a frame of reference, but rather the environment as it is cognitively or functionally responded to by the organism in the particular instance. In a certain way topological psychology is similar to the "Umweltforschung" of Uexküll. It deals, deliberately, not with stimuli or stimulus relationships, but rather with a pattern of reactions to be schematically located in 0, and from 0 dynamically onward until a new equilibrium is reached. Its chief merit is that it furnished an adequate conceptual tool for a description of this organized pattern or "field" intervening between the stimulating surroundings (C, B, A), on the one hand and the acted upon surroundings (A, B, C), on the other. Though quasi-spatial and highly generalized, topological psychology is not quantitative and not physicalistic in the usual sense. It enters the picture at a systematic locus symmetrical, or complementary, to the psychology of Brentano. Psychology of perception deals with the relationship of the world as it "is" for the organism in question at a certain moment, with the world as it "is" for the observing discursive experimenter or rational human being. Only the former is represented in topological psychology.

In conclusion: psychological research today presents itself as a pattern of fragments. These fragments tend to crystallize around the program of a gross correlational analysis in terms of achievement, converging "from above" with the disciplines dealing with molecular problems. Environmentalism seems to take the lead before mediationalism and molecular geneticism (as, *e.g.*, some of the questions of "explanation").

THEORETICAL AND EXPERIMENTAL DIFFICULTIES OF MODERN PSYCHOLOGY WITH THE BODY- MIND PROBLEM *

CHRISTIAN O. WEBER
Wells College

INTRODUCTION

H. C. Warren called the body-mind problem "the wandering Jew of science."¹ Having explicitly avowed its deliverance from philosophy, modern psychology proceeds to pile research upon research blissfully ignorant of the fact that this beggar was present from the first to bedevil its results. Philosophy has not solved the problem of the relation of mind to body; but it has at least acquired a healthy respect for the difficulty of the problem, well expressed in Durant Drake's exclamation, "Happy are they who can believe that the problem is a gratuitous one".²

Belatedly, psychologists are realizing that this problem is not gratuitous for psychology because it appears historically as a problem of philosophy. Psychology, whether it claims to investigate mental phenomena or behavior, deals with beings who apparently have both bodies and minds. Therefore, the body-mind problem is one of primary concern to psychology. The realization that this is the case arose as follows: With the accumulation of observational data psychology sought to formulate general laws. But univocal laws could not be found, because

* Sections IV and V of this study are slightly altered embodiments of an article originally published in the *Psychological Review* ("Valid and invalid conceptions of operationism in psychology," 49, 1942, 54-68). The author appreciates the kind permission of the editors of the *Review* to reprint the essentials of this article in the present larger whole.

¹ Warren, H. C. The mental and the physical, *Psychol. Rev.*, 21, 1914, 79.

² Drake, Durant. The mind-body impasse, *Phil. Rev.*, 32, 1923, 221.

the data obtained were influenced both by organismic and psychological factors. The resulting embarrassment is something like that of an astronomer who should find that the moon's path was affected by every responsive feeling aroused in the hearts of poets by moonlight. The laws attained by a psychology which considers both bodily and mental aspects of behavior turn out to be "hybrid laws". The most direct method for establishing univocal laws would be to confine psychology either to purely mental or to purely physiological processes. But, since physiological data is subject to quantitative statement the inevitable drift of psychologists has been to substitute physiological for mental phenomena in the hope of establishing an exact science.

The objectives of this paper are as follows:

1. First, to make clear the so-called "hybrid" character of psychological laws.
2. Secondly, to give a brief historical account of the body-mind problem which lies at the roots of such hybrid laws.
3. Thirdly, to give a more detailed account of contemporary efforts to attain univocal laws in psychology by means of a flight to physiology.
4. The fourth objective is to examine the value of operationism and relativism as devices for securing a monistic view in psychology.
5. Finally, an examination will be made of the attempts of certain logical positivists to solve the dilemma of body-mind dualism.

I. THE HYBRID CHARACTER OF PSYCHOLOGICAL LAWS

By a hybrid law I mean one which embraces effects which arise from factors which *differ in kind*. The law of the resolution of forces in physics also describes results which flow from two or more factors, but these are *alike in kind*. In general, the "likeness" of all physical factors rests in the possibility of measuring them quantitatively; and since the main task of physical science is conceived to be the attainment of quantitative laws, this quantitative measurability of all physical factors is enough.

There are, of course, secondary qualities in the physicist's world, but they never affect a physical process. Colors, for instance, play no role in chemical reactions — only the light vibrations rates which correspond to colors do so. If light rays of a given wave length fall on a photographic film the result is not

at all affected by the circumstance that you see such rays as red and the color-blind person sees them as yellow. Since physics may ignore the *quale* of things for this reason, they need not bother about the further difficulty that qualities, as such, cannot be stated in quantitative units of quality — that is the psychologist's problem. Psychology, seeking quantitative exactness, finds that the quantity of a sensation can be stated only in terms of the quantity of the stimulus which aroused it. The psychologist must deal with two realms, and the resulting laws of sensation will be hybrid laws.

In the case of sensory phenomena the hybrid character of the laws attained lies in the circumstance that the sensations aroused are in part an *effect* of the stimulus and in part a *reaction* to it. Consider the status of Weber's law of the relation between stimulus intensities and the intensities of the consequent sensations. If we sensed stimulus intensities at their true value, then sensations would show increments of intensity equal to the intensity increments of the stimuli. In this case, if we plotted stimulus intensities against sensation intensities we would get a straight-line relationship. Thus, where we have two equal sense distances a to b and b to c , the stimulus which aroused the sensation b would be the arithmetic mean between the stimulus intensities which aroused a and c . Merkel, indeed, found that the stimulus intensity for b does lie near the arithmetic mean. Fechner, however, claimed that the stimulus intensity for b is the geometric mean between those for a and c . Wundt met the predicament by supposing that there were two laws, Fechner's and Merkel's, and that they held under different conditions.

Now, Adrian shows experimentally that the response of the neurone to stimulus intensities follows a logarithmic pattern. If our subjective experiences of stimulus intensities corresponded to processes in the neurones affected, Fechner's law should hold. Yet, the actual sensation intensities which we experience lie nearer to the actual intensities of the stimuli. This implies, as Thouless notes, that a correction is being made.³ That is, your

³ Thouless, R. H. Phenomenal regression to the 'real' object: II, *Brit. J. Psychol.*, 22, 1931-32, 18-22.

conscious experience conforms more closely to reality than is accounted for by the findings of neurologists. But, as is usual in such constancy phenomena, the correction is not perfect. In short, the sensation intensity which we experience inwardly is a compromise effect between what the nervous system actually gives us and the true objective intensity. The psycho-physical law is therefore a hybrid law, affected by two constituents of human nature, one part of which operates physiologically and another part, which even though it has nerve processes of its own, still gives a *noetic* result lying closer to physical realities.

Since the advent of the study of constancy phenomena in perception, this hybrid principle has become familiar to psychology. When a white paper is seen in shadow it appears to have a brightness which is intermediate between the brightness recorded by the camera or photometer and the true brightness of the paper. The camera, being non-living, adds to the surface of the paper all of the darkness of the shadow. The observer, as though striving to attain the true brightness of the object, subtracts some of the shadow's effect: Not all of it, because the eye also follows the laws of physiological optics. Similarly, a man seen at a distance has an apparent size which is intermediate between his true size and the much smaller size which projective geometry yields. If you "fixate" this distant man visually and project the after-image, this image will be smaller than the image of a near object despite the fact that the man looks larger than this near object. In the same way the perceptual *form* of an object is a compromise between its true form and the form which projective geometry would give. In short, practically every result obtained in this field of research is hybrid, and appears to reflect the fact that we are at once body and mind. Add to this the fact that the extents of these compromise effects vary enormously from individual to individual, and we may conclude that such laws are "multihybrid".

This inter-penetration of the mental and the physical is illustrated by a multitude of proofs in the sphere of perception. For instance, perception is affected by the *attitudes* of the observer, and by the *meaningful* elements in perception. The rate

of reversals of so-called "reversible perspective" figures is partly under the control of the individual. In the sphere of memory the rate of learning depends on the presence or absence of meaning for the materials. As the materials learned become meaningless, the amount learned can more and more be accounted for by the objective laws of frequency and recency. As the material increases in meaningfulness and in good form these mechanical laws of learning become less important and the role of insight and *Gestalt* properties become more important. But, no learning becomes so completely mechanical that it can be accounted for completely by the law of frequency alone, whether this law is stated in terms of associational psychology or in terms of Pavlov's laws of conditioning. On the other hand, learning never becomes so insightful that frequency plays no role at all.

In the sphere of feeling we get a similar telescoping of the mental and the physical. In the olden days a soldier who fled the battle was shot at sunrise on the assumption that his cowardice was purely mental and therefore self-generated. Today, thanks to the work of endocrinologists, we are more inclined to send him to a base hospital for physiological examination. The truth, as McDougall argues, probably lies between these extreme views. The disordered gland might increase the soldier's susceptibility to fear, but his yielding to fear makes the gland's disorder worse. A full account of emotional life must recognize that every feeling is a resultant of both mental and organic factors. Maranon showed that the injection of adrenalin gives rise to states which to an extent simulate mental anxiety. Conversely, Cannon observed that when Harvard foot-ball men were told that they were to enter the game, the adrenalin concentration of the blood increased at once. A purely physiological account of emotion, it is assumed, would be simpler. Thus, Dr. Louis Berman, the physiologist, regrets that there are no photographs of Nietzsche which show him smiling. Not that the smile might throw further light on Nietzsche's outlook on life, but it would reveal the condition of his teeth! The trouble with this simplistic approach is that Nietzsche's outlook on life determines in part any effect that the condition of his teeth could have on

him. The role of *intention* and *attention* in the phenomena of psychology are indeed extensive. The most mechanical reflexes and habits may be generated, regulated, and terminated by them. The *Gestalt* psychologists have shown that learning does not occur readily where the intention to learn is absent. Yet, in all these phenomena final results are determined in part by physiological states of the organism. The results are hybrid because man is a hybrid creature, a blend of the mental and the physiological.

A sweeping objection to this bifocal view of man runs as follows: All of the instances I have cited by no means establish the existence of purely psychological determinants of the phenomena of sensation, perception, feeling, learning, etc. For, all of the effects mentioned may be regarded as consequences of purely physiological processes. For instance, Adrian's finding that there is a logarithmic relation between stimuli and consequent nerve processes holds for *peripheral* nerves. The tendency of observation to deviate from this to the 'true' intensity of the stimulus might be accounted for by other *central* nerve processes. In the same way one may assume special nerve processes which are the basis for the brightness correction for the object in shadow, the size of an object seen at a distance, and the form of an object seen at an angle.

Call the higher processes which mediate perceptual constancy corrections "nerve processes" if you will, but you now have two kinds of processes in the organism: One is the eye and its nerve elements which record impressions like a mere camera, deviating from the true character of the object, and other nerve processes which correct these impressions in *noetic* fashion. The chasm between such different nerve processes is as great as the chasm between body and mind with which we started. In the case of learning, any theory which seeks to explain learning in terms of neural mechanisms must perforce end up with the conception of *matter which can learn*. In contrast with this there exists the "ordinary matter" of every-day-life whose utter inability to learn gives physics all of its exactness. We end up with two subject-matters and two sciences after all!

The all-pervading character of purposeiveness in mental phenomena is the root principle of the *hormic* psychologies like those of McDougall and Freud. It is impossible, I think, to achieve an adequate objective account of purposiveness. John Dewey once suggested that a purely objective account of teleological behavior might be achieved as follows: Behavior consists of a succession of acts. We may regard the last act in the series as the 'end' and the preceding ones as the 'means'.⁴ The trouble with this view is that the objective time order of events will not always separate means and ends accurately. If one buys a piano on the installment plan, one's purpose is not to have something to pay for in the end! Every case of an alleged "objective" conception of purposiveness smuggles in a datum of subjective awareness at some point: the means-end distinction imposed upon the objective succession of acts. To be sure, there is what McDougall calls the extrinsic (objective) purposiveness of things. Thus, a savage who found a clock in the forest might infer that its purpose was to tell the time of day. But, this judgment is possible only because he entertains a subjective or intrinsic awareness that the clock could serve this felt or conceived *need*. The clock does not serve its own purpose — it can serve only the purposes of a being which can experience *felt* or conceived needs, that is, a *living* being, having a *subjective* life.

II. BRIEF HISTORY OF THE BODY-MIND PROBLEM IN MODERN PSYCHOLOGY

For modern experimental psychology the body-mind problem dates back to difficulties arising between the views of W. Wundt and F. Brentano. Wundt had maintained that the phenomena of consciousness constitute the proper field for psychology. These phenomena are: ideas, thoughts, imagination, volition, feeling, emotion, etc. The task of experimental method was to analyze these phenomena into their elements. The conviction grew that sensations were the irreducible constituents of mental life.

During the same year when all this was set forth in Wundt's *Grundzüge der Physiologischen Psychologie* (1874), Brentano

⁴Dewey, John. *Human Nature and Conduct*, 1922, 34.

published his *Psychologie vom Empirischen Standpunkt*. In this work Brentano drew a sharp distinction between *sensory contents* and *psychical acts*. Wundt had regarded sensations as the proper subject-matter of psychology. But Brentano, like modern neo-realists, considered that sensations were *physical* entities — only mental acts are the concern of psychology. This "subject-matter" turned out to be unsatisfactory, chiefly because such mental acts as perceiving, remembering, thinking, etc., can be described only in terms of their *contents*. To make a descriptive psychology possible, Titchener and Külpe, both students of Wundt, found a compromise between the views of Wundt and Brentano. This consisted in readmitting sensations to the sphere of psychology on the ground that physics and psychology start from the same world of experience. However, psychology was regarded as the study of experience regarded as dependent on the experiencing individual. Physics, on the other hand, makes its observations as if independent of the individual. Titchener thus secured a separation of the fields of physics and psychology *by creating a methodological dualism*.

The definition of psychology as the study of the experience of the experiencer supplied the grounds for a truly mentalistic science. Titchener, however, entertained the ideal of a quantitatively exact psychology. He cast longing eyes at physics which he lovingly refers to as "the mother of the sciences". How is psychology to attain such exactness? The most promising sphere for the establishment of such exactitude is the sphere of sensation. Units of sensation were sought with zeal, but the only ones attained were "just noticeable differences". Units of measurements are supposed to be equal, *i.e.*, substitutable, and j. n. d's. seem to fulfill this requirement, *but only for a given observer*. Imagine the corresponding predicament of physicists each with a meter stick of unique length! As for quantitative law, Weber's law was the only respectable one attained, and even this one is tied to stimulus intensities, and is of hybrid character, as we have seen.

One has to appreciate these difficulties of psychology in its attempt to attain the status of a quantitative and analytical science

in order to understand the drift of psychology towards physiology. The break which behaviorism made with introspectionism is being made by others in more gradual fashion. Albert P. Weiss, formerly hailed as the logical spokesman for behaviorism, wrote as follows: "The categories of mind, consciousness, awareness, purpose, volition, sensation, image, feeling, etc., have failed to establish the degree of phenomenological specificity which is essential for a uniform program of scientific investigation." Weiss then proceeds, like Zarathustra on his mountain, to intone a new set of postulates for psychology, introduced as follows: "In the set of postulates which follow, human behavior and social achievement are assumed to be forms of motion. . . . In other words, it is easier to agree on the properties of an electron or on the nature of motion,, than on the properties of a mental state or process." ⁵

III. MODERN TRENDS TOWARDS PHYSIOLOGICAL PSYCHOLOGY

I turn now to this blind impulse to measure at all costs as manifested in contemporary psychology. In many of our one-time introspective psychologists the attempt to secure the possibility of exact measurement is revealed in three contemporary trends, as follows:

1. The growing interest in physiological and neurological data as the chief subject-matter of psychology.
2. The ordering of the findings of psychology under the rubrics of 'fields' and 'dimensions'.
3. The advent of a new logic of method for psychology known as *operationism*.

I will discuss the first two trends in the present section, reserving operationism for special treatment in the next section. I will illustrate what is happening to the introspective psychology of Titchener in the hands of one of his most prominent students, Edwin G. Boring, whose point of view is set forth in a book bearing the paradoxical title, *The Physical Dimensions of Consciousness* (1933). Boring's position may be summarized by saying that he accepts dualism for the present, but looks forward with

⁵ Weiss, A. P. One set of postulates for a behavioristic psychology, *Psychol. Rev.*, 32, 1925, 83-84.

some confidence that psychology will attain a monistic view. Mental and physiological processes are not as yet shown to be the same thing. The task for the present is to investigate their parallelism as exhaustively as possible. During this time, physiology is to have no prerogatives over psychology. That is, "a physiological theory of neural action ought not seriously to be maintained if it is incompatible with psychological fact, and that psychology stands just as ready to provide sanctions for neural theory as physiology is prepared to render a symmetrical service to psychology."⁶

At the same time Boring conceives introspective method as very similar to physical observation. For physics and psychology alike all concepts are the results of inferences from data. Boring rejects the view that simple sensations are the results of immediate experience". The sensations, their attributes, perceptions, feelings, and so on, are inferred from the data of experimental work. The "verbal report" of the observer is a *mediate* experimental procedure. The significance of this view is that it makes the procedures of psychology as public and objective as are the procedures of physics.

I now turn from the psychology which Boring perforce accepts for the present to the psychology which he hopes we may achieve. The ideal, he says, "would be ultimately to get away from conscious dimensions to physical dimensions, to the happy monism of the scientific heaven."⁷ Again, he says, "The ultimate abandonment of dualism leaves us with the physical world as the only reality."⁸

How is this identification of the mental with the physical to be brought about? By demonstrating an ever closer and closer correlation between mental and neural events. "To the author a perfect correlation is identity. Two events that always occur together at the same time in the same place, without any temporal or spatial differentiation at all, are not two events but the same event."⁹ Boring gives this statement in a footnote, as

⁶ Boring, E. G. *The Physical Dimensions of Consciousness*, 1933, preface, p. vi.

⁷ Boring, E. G. *Op. cit.*, preface, vii.

⁸ Boring, E. G. *Op. cit.*, 8.

⁹ Boring, E. G. *Op. cit.*, footnote, p. 16.

though a little afraid of it. It is puzzling to understand what is meant by a correlation where there is but one variable. We would not say that temperature and the height of the mercury column are identical, although the correlation between them is about as perfect as we could hope for in this world. Still, one must remember that the followers of Bridgman in physics would define temperature as identical with the operation of measuring it; and Boring, as we shall see later, favors operational definitions.

Boring grants that for the present we are not ready to give up conscious dimensions in psychology. Quality, extensity, intensity, and protensity are indispensable. The bulk of Boring's book is occupied with the attempt to treat these psychological dimensions as physical ones. The possibility of this reduction seems plausible in view of certain considerations. Consciousness is essentially *discriminative*. Discrimination implies differentiation, and the nervous system makes differentiation possible. "Consciousness is *localized in the brain* in the sense that discriminative specificity originates there within the differentiated field that may be imposed by the periphery. . . . *Discrimination* may ultimately be *spatial*, for introspection can be reduced to a selection between two neural paths. . . . In this sense all the dimensions of consciousness require place theories."¹⁰

But, when Boring seeks to account for quality, intensity, protensity, and extensity as forms of 'place' discriminations, the results, as he frankly admits, are disappointing.

I criticize, not Boring's failure to establish his identity hypothesis empirically, but his assumption that psychology is bound to try to make this identification. His entire project is logically unpromising, and this is why it is likely to be an empirical failure also. I will overlook the consideration that such 'dimensions' as quality, intensity, and the like are inadequate for a full description of consciousness. Also, let us concede that the definition of consciousness as the realm of discrimination is sound, which it certainly is so far as it goes. Further, discrimination does imply differentiation, and no doubt the nervous system plays an important part in making differentiations possible. Grant

¹⁰ Boring, E. G. *Op. cit.*, 234.

also that the nervous system makes differentiations by means of processes in different areas of the brain. *Still this does not account for qualitative differentiation.* Boring confesses that of the four conscious 'dimensions', that of *quality* is the most difficult to subsume under a 'place' theory. Suppose that the observer discriminates a red area from a blue one. It might appear that this discrimination is possible because these areas are spatially separate. But it is equally true that the areas are seen as separate *because the red and the blue are discriminated.* As regards events in the cortex, a place theory would seem to have a certain plausibility if one includes time as an additional factor in discrimination. That is, when you see two adjoining red and blue areas in outer space, we can say with confidence that two neighboring areas of the visual cortex are being excited. If we see red and blue succeed each other in the same place, the cortex may now provide a succession of processes as the basis for the discrimination. All of this helps to explain *the means by which* red and blue are discriminated, but it does not constitute the *quale* of the red and blue. No doubt the cortical processes underlying the experiences of red and blue differ, *but these processes are not themselves red and blue.* Boring's hope to reduce consciousness to physical dimensions is the futile attempt to bridge the gap between things which differ in *kind* by steps which differ in quantity only. I doubt whether the Rubicon between matter and mind can be crossed in this way.

Professor W. E. Hocking had the following to say about this matter: "The body is spatial, whether for the old physics or for the new; it has relations of distance to objects outside of it; its parts are near and far, above or below each other. This is not true of the mind or of any mental event. . . . We must persistently remind ourselves that the thought of a distant object is not a distant thought, nor the thought of a twisted object a twisted thought. . . . There is probably some peculiar mark in the brain which distinguishes a memory dated backward, from an anticipation of the same thing dated forward. . . . But such marks are *present* marks. The translation of such marks into true time-distance remains a prerogative of the mind; for the

mind alone . . . has a grasp of the past in its own nature along with the present and the future.”¹¹

The futility of the attempt to render an account of mental events in terms of brain physiology is especially apparent when we are attempting *description*. From the *causal* point of view there are no doubt innumerable and discoverable interdependencies between mind and nerve physiology. The basic difficulty appears when we try to account for one of these realms as *constituting* the other. Suppose that an investigator from Mars were to establish a Psychological Institute on earth, and suppose further that like the other denizens of Mars he can never feel love. Nevertheless, he discovers certain characteristic electrical discharges, Berger rhythms perhaps, in the brains of his earthly subjects. There are good prospects, that this investigator may end up with several volumes of data from which we learn that these cortical discharges show characteristic disturbances during adolescence, that they are peculiarly unstable in the region of Reno, and so on. But no end of volumes of such data can bridge the gap between appearances and realities.

Professor Boring, of course, is not alone in his hope to substitute physiology for psychology. I select his case not because his success is bad. On the contrary, in view of the redoubtable problem he confronts, that of reducing consciousness to physical dimensions, he succeeds as well as any one could. One is reminded of the observation of William James that professors are likely to defend improbable theories because these are the ones which show their ingenuity best, and give them the comfortable feeling that they have earned their salaries!

I will now present the views of one more belated disciple of Weiss. C. C. Pratt takes his departure from the failure of every attempt to distinguish the mental from the physical. The entire history of the disputes of philosophers and psychologists alike appear as a welter of conflicting views. Since no operational criteria have been discovered which distinguish mental from physical events, the nature of all events is ontologically the same.¹² Now, strangely enough, of two indistinguishable

¹¹ Hocking, W. E. *The Self, Its Body and Its Freedom*, 1928, 28-33.

things, Pratt selects the physiological as the proper subject-matter for psychology. He writes, "The principal task of theoretical psychology in making a scientific portrait of human nature is to discover the immediate antecedents of initial descriptive data. These antecedent conditions are located within the biological organism. . . . The theoretical importance of psychological descriptions, as contrasted with any practical significance they may conceivably possess, derives almost exclusively from the light they throw on physiological mechanisms".¹³

I offer some brief criticisms of Pratt's view as follows: In the first place, the negative finding that no one has distinguished between the mental and the physical in a way acceptable to everyone else cannot serve as premise for the positive conclusion that the nature of all events is ontologically the same. Pratt seeks to strengthen his conclusion by the adoption of operationism, and he refers approvingly to the positivistic school of logicians. But, the logical positivists maintain that a distinction not subject to operational investigation is *meaningless*, not that meanings which cannot be distinguished are identical.

I challenge the assertion that there are no operational methods for separating the mental from the physical. I would say that psychology must remain distinct from physiology because introspective method and physiological method are different and yield different data. By introspection I know of the existence of a vast host of sensations, feelings, purposes which no physiologist has yet discovered floating before the objective of a microscope or confined in a test tube. On the other hand, I never discover in my introspective experience the physiological processes which the physiologist says are the causal conditions of these phenomena.

The trend towards physiology is said to arise from the circumstance that the elementary experiences of mind are indescribable. To know what red is one has to experience it, and psychologists are not satisfied with a speechless role! I grant that colors just are what they are — their *qualé* cannot be em-

¹³ Pratt, C. C. *The logic of Modern Psychology*, 1939, preface, viii.

¹⁴ *Op. cit.*, preface, ix-x.

bodied in words, but it does not follow that this renders psychology mute. There is plenty to say about the relations between these mute experiences: red with bluish green gives gray, with blue it gives violet and purple, its quality varies with its intensity. We have the realm of qualitative principles resulting from studies of fusion, contrast, after-images, and the effects of configuration upon all of these. There is no objection to tracing the relations between colors and light rays provided we do not identify them. No, I believe that the motive which leads Boring, Pratt and a small army of others to persist in this attempt to identify mental with neurological events lies deeper. It is the inability to rest content with Aristotle's dictum that we should expect no more exactness from any field of study than its own nature is capable of yielding. It is not enough that psychology is partially quantitative — we long for a wholly quantitative science. Always there is the implication that when we know more we will know that the mind is a quantity. I reply, we cannot say at present what we will know when we know more!

So far we have seen how the psychophobia of psychologists leads them to favor physiology, and to favor physical concepts of fields and dimensions as devices for ordering data. I turn now to the adoption of so-called *operationism* and relativistic points of view by means of which psychology hastens its flight from the psychological.

IV. OPERATIONISM AND RELATIVISM IN PSYCHOLOGY

As regards the advent of operationism in psychology it will be well to give a preliminary account of P. W. Bridgman's ideas as set forth in his well-known book, *The Logic of Modern Physics* (1927). Bridgman makes it clear that his adoption of operationism was a consequence of the appearance of relativism and quantum theory in physics. It was a great "shock" Bridgman says to discover that the conceptions of classic physics were so out of harmony with the facts of nature. Looking to the future of science, Bridgman anticipates that our views of nature must always be subject to change; but "there is a part of our attitude

to nature which should not be subject to future change, namely that part which rests on the permanent basis of the character of our minds. It is precisely here, in our improved understanding of our mental relations to nature, that the permanent contribution of relativity is to be found. We should now make it our business to understand so thoroughly the character of our permanent mental relations to nature that another change in our attitude, such as that due to Einstein, shall be forever impossible."¹⁴

Operationism is the point of view which Bridgman offers to prevent the 'shocks' which confronted classic physics in the form of relativity. Operationism is that "improved understanding of our mental nature" which need not be subject to further change. Bridgman defines operationism as follows: "To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*"¹⁵

Bridgman reveals quite frankly the motives which lead him to the adoption of operationism. The appeal of operationism is due to its strategical value for avoiding *ontological* problems. For, if at any future time nature fails to show the attributes implied by our concepts we can say that in fact the concepts refer, not to nature, but to our operations on nature. If electricity is defined as that which the galvanometer measures we are relieved of the embarrassing task of deciding what electricity is in its own nature. For the psychologist, if intelligence is defined as that which the intelligence test measures, a similar Gordian knot is cut.

I wish next to call attention to two ambiguities which appear in Bridgman's definition of operationism. The more ordinary theory of knowledge is triadic: That is to say, we assume three things, the object (nature), our operations on the object, and the concepts resulting from the operations. A clear-cut oper-

¹⁴ Bridgman, P. W. *The Logic of Modern Physics*, 1927, 1-2.

¹⁵ Bridgman, P. W. *Op. cit.*, 5.

ationism would reduce this triadic relation to a dyadic one: We may ignore the object, and concern ourselves only with the concept and the operation which yields it. The operationist need not declare that the object does not exist, but only that the object may be ignored in the pursuit of knowledge; for operationism, if unambiguous, maintains that the operation determines our knowledge (concepts) completely. And yet, Bridgman does not say that nature can be ignored. In one isolated passage at least he states that the fundamental operations we may use are determined by nature,¹⁶ and in a final chapter Bridgman discusses "special views of nature" — the question of her simplicity, determinism, and other abstract characteristics. His views regarding the role played by nature in our pursuit of knowledge are therefore ambiguous and hazy.

The second ambiguity appears in Bridgman's definition of operationism itself. Note that he does not say that the operation and the concept are *identical*. Had he done so, then nature might be ignored entirely in our pursuit of knowledge. But, instead, he says that the operation and the concept are *synonymous*. Now, synonymous meanings may include some which are *nearly the same*. The situation seems to be that we can ignore nature but not quite completely, because the operation may not determine all of the concept; or, what is worse, nature sets limits upon the operations we may select for investigating her. In short, Bridgman seeks a point of view which will deliver us from the 'shocks' which nature gives us by failing to conform to our concepts, and yet he wishes to believe that our concepts reveal the true attributes of nature!

Bridgman shares with the logical positivists the view that questions which have 'no corresponding operation which could answer them, are meaningless.' This is logically consistent with the view that operations and concepts are identical. If concepts are regarded as referring to nature we would speak of them as *true* or *false*. But if the operation *is* the concept, then where there is no operation there is no meaningful concept.

¹⁶ Bridgman, P. W. *Op. cit.*, 29.

¹⁷ Bridgman, P. W. *Op. cit.*, 29.

The fundamental difficulty in Bridgman's view is whether operationism will satisfy the motive for adopting it. He adopted operationism to avoid the 'shocks' which nature inflicts on us by failing to conform to our cherished concepts. My criticism is that if operationism is adopted these 'shocks' merely pass into the 'shock absorber'. That is to say, perhaps many physicists love their operations more than they love nature; and it will be shocking to find a new operation which entirely discredits an old one! Poincaré says somewhere that science sometimes clarifies nature by shutting up all of the darkness within its textbooks! Bridgman's view seems to transfer the contradictions which arise between concepts and nature to contradictions which appear between operations. The hiatus between the concepts of relativism and those of classic physics is reflected by a similar hiatus between the methods of relativism and those of classic physics.

Before leaving Bridgman I wish to note one way in which he uses operationism which seems to be excellent. This may be illustrated by the measurement of space. When we measure the length of a room there are grounds for believing that we arrive at a concept which reveals a true attribute of the room, *i.e.*, its spatiality. This is because the space of the room is gauged in terms of multiples of a ruler *which also has the attribute of spatiality*. The distance of the moon, however, cannot be measured directly. This distance could be determined by triangulation from two widely separated points on the earth; but now *light* plays an important role. To measure the distance of points outside the solar system we resort to measurements of parallax. Still greater distances are measured in terms of light years. At this point we are not sure that we are measuring pure spatiality at all! In this case it is therefore a wise precaution to say that the distances of such points have reference to the methods by which they were measured. This is merely to warn us of the possibility that the operation did not measure space in the ordinary sense. This narrower use of operationism is excellent; but Bridgman, with the thirst for sweeping generalization which scientists unknown to themselves share with philosophers, universalizes the

principle, and claims that operationism applies as well to the original measurement of the room!

Operationism in Psychology.—Usually, psychologists define operationism in less hazy fashion than does Bridgman. McGeoch proposes "that learning can be more adequately defined in terms of the operations of measurement than in terms of phenomenal properties".¹⁸ McGregor defends operationism defined as the "belief that an entity is adequately defined only in terms of the specific operations involved in its observation".¹⁹

The psychologist's use of operationism to escape the entanglements of ontology may be illustrated by two examples. Consider Boring's treatment of our experience of duration and of temporal patterns. These experiences are notoriously subjective and inconsistent with clock-time. It would be a blessing if we could rid these experiences of their subjectivity and thus avoid the need of characterizing them as mental processes. Boring accomplishes this by adopting an operational definition of duration as follows: "A perceived duration or temporal pattern is defined by the operation of introspective report by means of which it is studied. It need not be regarded as an entity, but as a verifiable and public construct. The concept of duration is an *inference* from introspective reports, and duration as an immediate experience is meaningless."²⁰

Another example is from Stevens who maintains that for both physics and psychology the act of *discrimination* is fundamental. Discrimination is an operation which points to or denotes something. Equality means the absence of discriminable differences. When operations fail to reveal a discriminable difference, we have equality. Thus, we do not ask whether objects or processes are equal to each other in themselves—equally refers to the operation of discrimination, not to the objects discriminated.²¹

¹⁸ McGeoch, J. A. Learning as an operationally defined concept, *Psychol. Bull.*, 32, 1935, 688.

¹⁹ McGregor, D. Scientific measurement and psychology, *Psychol. Rev.*, 42, 1935, 246-266.

²⁰ Boring, E. G. Temporal perception and operationism, *Am. J. Psychol.*, 48, 1936, 519-522.

²¹ Stevens, S. S. The operational basis of psychology, *Am. J. Psychol.*, 47, 1935, 323-330.

We have seen how Bridgman invokes operationism to meet the difficulty raised by relativity regarding the existence of an independent realm of objects. In the physical world, an object no longer has an independent existence of its own. Its traits arise largely if not entirely from other objects which surround it. A given body has mass only because other bodies exert gravitational pull on it. An electron appears more and more to be merely a nexus of relations sustained between other bodies which surround it, which in their turn are described in the same way. In place of Aristotle's dictum that *everything is what it is* we are led to say that *everything is something else*! Consider how this relativity of things bedevils the psychologist who wishes to show that there are mental processes which have a circumscribed existence of their own. Carr, for example, has examined the merits of learning things by wholes with learning them part by part. He concludes that the method which is the more efficient varies with each variation of the situation: The age, training, memory span, I. Q. level of the subject, the form of the material learned, the method of learning it, etc. These facts suggest to Carr the advisability of adopting a relativistic point of view in psychology — that all results are relative to the methods and conditions used in attaining the results.²²

It will be well to begin criticism of operationism in psychology by noting certain objections to it which seem to me to be invalid. The most obvious of these is to reject operationism because of our habitual and sentimental attachment to the time-honored triadic view that a theory of knowledge must assume three things: a realm of objects, operations made on these objects, and concepts resulting from the operations. The critic will say that operationism is invalid because operations are only *adequate* or *inadequate* — we cannot say that they are *true* or *false*. Only concepts may be true or false, since they alone refer to nature. We have here merely the assertatory acceptance of the triadic theory. The objector may argue that the entire history of inductive logic and of empirical science assumes the existence of objects which determine what the nature of the concepts must be.

²² Carr, H. A. The quest for constants. *Psychol. Rev.*, 40, 1933, 514-532.

Of course, it may be the very purpose of operationism to break down this historical view, and we cannot allow the mere attempt to do this as proof of its own invalidity! The length of time during which a point of view is accepted creates a presumption of its truth but does not establish its certainty. If the invalidity of the dyadic view (operationism) is to be established, we cannot use data from the history of science if this history shows no serious attempts to use it.

A second objection consists in saying that concepts contain much more than operations can yield. A physical instance of this objection would be to say that our concept of the room's length is not given entirely by the technique of measurement. The operation with the ruler yields a number, but that this number refers to *length* is not given by mere counting. Surely the room had length before we measured it. The ruler gives us discontinuous units, but we believe that the length of the room is continuous. The same Bergsonian objection may be made regarding measurements of time. This argument invokes Aristotle's distinction between the measured and the measuring time.

I will cite a similar instance of this type of objection to operationism from psychology, namely, in regard to color contrast. The purpose of the objection is again to show that the concept contains more than is indicated in the corresponding operation. Color contrast appears whenever we view any colored surfaces whose contours are hazy. Here it seems obvious that the operation determines only the *conditions* under which contrast appears. The contrast *experienced* is not in the stimulus conditions at all, but in the observer.

Operationism, however, may reply to all this by saying that the objections hold because operations are defined too narrowly. Admit that we know what the length of the room means because we *traverse* as well as *measure* it, but the traversal of space is but another operation. The belief that the room is continuous may be regarded as the result of rational operations rather than with operations with a ruler. Thus, Zeno's paradoxes are logical arguments (operations) refuting the notion that space could be composed of an infinity of spaceless points. No doubt we

need more than laboratory equipment to demonstrate color-contrast; we need an observer who adds an essential operation of his own. The color-blind man will not see color-contrast — he is incapable of the necessary operation.

The critic of operationism may declare that the realm of immediate experience lies beyond the sphere of operations; and that the term 'operation' seems an inadequate designation for experiences so immediate as the seeing of a color or the hearing of a tone. This is the reason why Boring rejects immediate experience as a psychological source of scientific data. He wishes to confine science to mediate operations because only these are, so to speak, public. The material sciences too must infer the nature of their objects from public or verifiable observations, which also have their origin in 'immediate experiences'. Psychology is in no greater difficulty in this respect than any other science.

Another charge against operationism is that it would inevitably lead to the uncritical multiplication of methods. Waters and Pennington write, "The slightest change in any aspect of a set of operations would mean . . . a new concept, and would demand, likewise, a new symbol for its designation".²³

It is true that Bridgman demands that each set of operations be a *unique* set, but one may reply that *common denominators of method* will be found to exist among operations. Thus, in physics, the specific heat of a gas may be determined by the direct method of literally heating the gas and measuring the rise in temperature. Or, the speed of sound may be determined in the gas before and after heating it. Calculations from both sets of data will yield the same measure of specific heat for a given gas. In psychology, color contrast may be studied with colored papers and tissue paper, with a color motor, by viewing the colors in twilight, by viewing them at a distance. In all four of these methods the common operational feature is that color contrast appears when the contours of the stimuli are obliterated. In this case it does not follow that the adoption of operationism will

²³ Waters, R. H. and Pennington, L. A. Operationism in psychology, *Psychol. Rev.*, 45, 1938, 414-423.

lead to an inordinate multiplication of concepts or operations; and I think that the same may be demonstrated for other fields of research in psychology.

We arrive now at the crucial objection which I would urge against the form of operationism which identifies the operation with the concept, and seeks by this means to escape ontological questions. Our conviction regarding the existence of a realm of physical objects and of mental processes that lie *beyond* the operations arises from a certain *necessity* or *requiredness* which they impose on us. When the traditional scientist attained the same results by quite different methods he was doubly sure that he had discovered the true lineaments of nature. Why? Because of this evidence of the existence of a realm of brute fact which determines (1) what methods we must use, and (2) what the results will be. Thus, we cannot choose to measure electricity with a gas meter. *Instead of freely selecting the instruments with which to operate on nature, we must perforce select instruments upon which nature will operate.* Now, my point is that operationism cannot ignore this external realm of necessity, for all of the necessity will appear in the realm of operations. In the case of measuring the specific heat of a gas, it makes no difference as far as *requiredness* is concerned whether we say that there is some ontological necessity which makes the sound conductivity of a gas vary with its heat; or whether we say with the operationists that there is a necessary connection between two ways of measuring the specific heat of a gas. The traditional physicist was led to believe in a realm of reality consisting in the 'systematic interconnection of things'. The operationist will discover the same degree of systematic interconnection among his operations.

It is only when the operationist forgets this aspect of necessity that we may fear that the multiplications of methods will run riot. Forget the mutual implications existing between methods, and each method is as good as any other. When an astigmatic person looks at an object he may see several overlapping objects. This is interesting as a consequence of the operation by means of which an astigmatic person sees things. But if one

adopts the brand of operationism which ignores the necessary interconnection between operations, the consequence would be that the normal man who sees but one object is no more right than the astigmatic subject: the concept is a function of the method! Of course, the operationist knows this full well, and also realizes that a proper understanding of the properties of lenses will explain why the normal man sees one object and the astigmatic man sees several of them. But this is to grant that there is necessary and systematic interconnectedness in the realm of operations no less than in the realm of nature. What then has been gained by adopting operationism? It has been adopted to escape the necessity of characterizing the ontological natures of physical objects and of mental processes. The ontological nature of things may be a nuisance, but they are no less so when entrenched in method than when they are entrenched in nature.

Operationism conceived as the identity of operations and concepts introduces a certain incompleteness in our logical modes of thought in psychology consisting in its failure to provide for both connotative and denotative definition. Seashore and Katz nicely illustrate the confinement of operationism to extensional definition.²⁴ Psychological operations, they say, often consist in two steps: We may, for instance, deal with the sensory quality of red as follows: We first indicate the stimulus which will produce the experience of redness. After putting the subject through the necessary operation, the second step consists in saying, "That is the color red".

It is clear that the entire process here is one which *points to* the object but does not give its denotative attributes. We cannot object to this failure. I doubt whether red can be defined in a denotative way. In vain will you employ words to make a blind man realize what the quality red is like. Grant that this is because the blind man cannot perform the necessary operation, but why not admit that the connotative attribute of redness must come as an immediate experience? The only thing is the way of doing this is the pedantic insistence that psychology must

²⁴ Seashore, R. H. and Katz, B. An operational definition and classification of mental mechanisms, *Psychol. Record*, I, 1937, 3-24.

employ but one kind of operation, *mediate ones*, like those in physics. To be sure, to admit into psychology two forms of operations, immediate and mediate ones, implies the very dualism of body and mind which psychologists wish to escape. I do not think they can escape it without paying a price. Immediate experience alone can give the subject the experience of redness. The mediate operations of Boring's introspective reports can make the experience 'public' and scientific. The further mediate operations of neurologists may reveal the electrical processes in the optic tracts and areas which somehow condition this experience. We should, as McDougall says, work both of these points for view for all they are worth, and forget this petty demand for simplicity.

The limitation of operationism to denotation appears in physics as well as in psychology. The statement that electricity is that which the galvanometer measures at best *locates* but fails to characterize the nature of electricity. The nature of electricity must of course be stated in terms of the effects which electricity has on matter. One cannot escape this ontological reference by the adoption of operationism, because measuring instruments too are physical things. So we have, after all, the Newtonian logic which says that the properties of things are revealed by their interactions on each other.

V. OPERATIONISM AND LOGICAL POSITIVISM

Sometimes psychologists introduce their books with a most emphatic rejection of all philosophy, and then proceed to list, as Weiss does, a set of metaphysical assumptions abstract enough to render Descartes green with envy! Titchener is said to have observed that behaviorism is a consequence of the ignorance of history. This ignorance of history is shown by operationists in psychology who unknown to themselves hold to well-known philosophical views. I will trace the resemblance of operationism to the doctrines of logical positivism in philosophy as set forth by Rudolph Carnap.

Logical positivists manifest the desire we have noted in

operationists to dodge ontological questions. Carnap proposes to achieve this by translating all predicative statements into syntactical statements. Syntactical statements have to do only with the *form* of the assertion, not with its *material content*. Thus, the material mode of speech, "the rose is a thing", becomes, "The word 'rose' is a thing-designation". In the same fashion material statements regarding relation, number, casualty, modality, may be translated into syntactical form.²⁵

Carnap's specific purpose is to attain a systematic position which will enable the philosopher to dodge the question of what things really are. He declares that if we do not avoid the material mode of speech "we shall find ourselves sliding into the midst of metaphysics — and that is sliding into the mud".²⁶

This position is far more defensible for a philosopher than it is for a psychologist. In a true sense, questions of the existence and nature of things (whether these are physical or mental entities) *are* the concerns of science, not of philosophy. Carnap is seeking to establish a clear-cut sphere of activity for philosophers. He conceives this to be the analysis of the languages of the sciences. There is no philosophy of nature, but only the philosophy of natural science; no philosophy of biology, but only the philosophy of biological science, and so on for all of the sciences.

In this same fashion operationists might conceive it to be their task to make a systematic study of the *methods* of science *as such*, a task which would free them from the labors of concrete discovery and would leave them in the sphere of logic. But we have seen that their intention was quite the reverse: They intended to free themselves from philosophy in order to give all of their energies to the application of operations in the laboratory. Carnap, after giving wise council which should keep philosophers out of the laboratory, proceeds to set a bad example. This is shown by his defense of *physicalism*, a doctrine for which Dr. Neurath is particularly responsible. Physicalism maintains that the language of physics is the basic language for all sciences.

²⁵ Carnap, R. *Philosophy and Logical Syntax*, London, 1935.

²⁶ Carnap, R. *Op. cit.*, 95-96.

That is to say, every sentence of any branch of science can be translated into the language of physics. I call Carnap's example a 'bad' one, because after rejecting all ontological questions he inconsistently accepts physical statements as the basis for all other statements. I will illustrate his manner of translating psychological statements in particular into physical form.

Carnap gives the following example: He states that the psychological sentence, "At ten o'clock Mr. A. was angry", may be translated into the physical sentence, "At ten o'clock Mr. A. was in a certain bodily condition which is characterized by the acceleration of breathing and pulsation, by the tension of certain muscles, by the tendency to certain violent behavior, and so on".²⁷ With the help of a certain scientific principle, says Carnap, we may reverse the last statement and say that whenever Mr. A. is in the bodily state mentioned he is angry. Of course, Walter B. Cannon would reject this simple conversion, for his researches fail to show that there is a one-to-one relation between *felt* emotions and their bodily accompaniments. This would not invalidate Carnap's proposal in general; for, if Cannon is right, there is no scientific principle for making the conversion. Carnap clearly wishes to make such conversions when they are justified empirically. If the mental state has no observable bodily accompaniment, Carnap maintains that such mental states cannot be verified at all. Mr. A. may give a report concerning his felt anger, but Carnap notes that such reports are observable.

In the terminology of logical syntax, statements which have the same meaning are 'equipollent'; and Carnap regards the physical translation in the case of Mr. A. as equipollent to the psychological statement that Mr. A. is angry. The term 'equipollent' serves a function similar to that of the term 'synonymous' in Bridgman's definition of operationism. Not wishing to exclude nature entirely from the concern of science, Bridgman says the operation is synonymous with the concept, not *identical* with it. Carnap would fain believe that the physical and psychological sentences regarding Mr. A. are identical, but regard for the *realities* of the case lead him to substitute the more timorous word

²⁷ Carnap, R. *Op. cit.*, 89-90.

'equipollent'. If we give consent to the facts, when a subject is angry he both *feels* and displays certain organic states. These processes are so unlike each other that we have no way of bringing them together except to note their temporal and 'place' associations. They are related to each other *causally*, but from the *constitutive* point of view their glaring lack of identity is ill concealed by the use of the cryptic word 'equipollent'. Which one of these processes or *statements regarding* processes is the more significant for psychological science? Surely, both are significant, but to accept both we must be content with dualism. Carnap's system of translating material statements into syntactical statements may avoid the mud of metaphysics, but he does not solve the problem of dualism except by the *tour de force* of rejecting psychological statements in favor of physical ones.

Operationally considered, the method by which the subject learns of the existence of *felt* anger is radically different from the physiologist's method of studying it. Like Carnap, behaviorists quite generally throw introspective method overboard and end up with the statement that the bodily state *is* the emotion. This is done because the bodily state is presumed to be more 'objective'. In the case of Carnap and of behaviorism ontological interests may be denied, but they succeed nevertheless in determining the type of operations which will be accepted. This arbitrary violence in the realm of scientific method is not so likely to occur if the operationist will remember that there is a realm of realities which determines what his choice of operations must be. If two equally valid operations point to the existence of two radically different realities, let us for the present accept the fact rather than yield to the desire of philosophers for monism. I should like to cite a statement from Bridgman which it is all too common to skip over. "The fact has always been for the physicist the one ultimate thing from which there is no appeal, and in the face of which the only possible attitude is a humility almost religion."²⁸

²⁸ Carnap, R. *Op. cit.*, 27.

SUMMARY

Organic and psychological processes are so inextricably entwined that the laws of traditional psychology are hybrid laws in the sense that they reflect the influences of both the organic and the mental constitution of the individual. This is shown to be the case in the spheres of sensation, perception, emotion, and for goal-seeking (teleological) behavior. The dilemma of psychology is that it must either rest content with this hybrid character of its field and findings, or seek to achieve a monistic point of view.

But the attempt to break down traditional body-mind dualism always results in a dualism of some other form. The opposition between Wundt and Brentano regarding the ontological status of sensations was solved by Kulpe and Titchener at the cost of accepting a methodological dualism. The behaviorists and even certain psychologists of the introspective tradition (Boring and Pratt) try to secure a monistic point of view by accepting physiological data as more fundamental than psychological statements and by seeking to order the findings of psychology by means of concepts of dimensions and fields. It is shown, however, that these devices achieve monism by doing that violence to facts which consists in neglecting their differences.

The adoption of operationism as a device for *avoiding* ontological questions entirely is subject to critical scrutiny. The brand of operationism which identifies the concept with the operation is rejected. The dualism in nature which it seeks to dodge reappears in the realm of operations. Operationism may ignore the systematic order or lack of it in the things upon which operations are made; but the systematic order or lack of it will reappear among the operations. The crux of the difficulty in the way of a monistic psychology is that the interlocking system which appears among physical operations differs from that which appears in psychology. The introspective method which for example reveals experienced feelings is as far removed from the physiological observations which fail to reveal feelings as body was removed from mind in the first place. If the Rubicon between matter and mind cannot be crossed it does not help much

to say that the difficulty lies in the operations for crossing it!

Rudolph Carnap's syntactical approach replaces the dualism of nature by a dualism of inconsistent physical and psychological 'sentences'; and he is obliged to solve the problem as the behaviorists do by regarding the physical statements as fundamental. The dense fog of Hegelianism resulted from the attempt to identify reality with ideas about reality. The assumption that operations are identical with concepts has all of the ingredients necessary for a similar fruition of metaphysical verbiage.

The psychologist who adopts operationism will do well to regard the concept as derived *by means* of the operation. As various methods for measuring a given reality are perfected they will tend to yield the same concepts. Thus concepts become more and more independent of any one method, and conform more and more to the nature of reality whose existence is also independent of method. However, when concepts are new, we must take special care to remember their derivation from and dependence on the methods by which they were secured. This simple warning embodies the chief value of the recent advent of operationism in psychology. The warning is not superfluous for numerous and futile debates have their origin in the failure to realize that many contradictory findings are conditioned by the methods used in securing them.

Ernst Mach somewhere observes that if we are willing to ignore differences a penny and the moon may be regarded as identical. This is the only way, so far, in which monism has been achieved in psychology! Almost invariably monism has been sought by neglecting mental phenomena because they are not completely measurable. In reply to all such attempts I can do no better than to cite a comment from Yule, an authority on measurement. He writes, "Failing the possibility of your measuring that which you desire, the lust for measurement may, for instance, merely result in your measuring something else — and perhaps forgetting the difference — or in your ignoring some things merely because they cannot be measured".²⁹

²⁹ Yule, G. Undy. In a critical notice on "The Essentials of Mental Measurement" by W. Brown and G. H. Thomson, *Brit. J. Psychol.* 12, 1921-22, 107.

PERSONALITY AND TYPOLOGY

ISIDOR CHEIN

College of the City of New York

This paper was undertaken in the hope of contributing to a clarification of some of the issues confronting the student of personality. Specifically, we shall consider the problem of the nature of personality and the problem of the typological approach to the study of personality.

From the viewpoint of systematic psychology, the first question to be answered concerns the place of personality study in the field of psychology. It is evident, from even a cursory examination of the psychological literature, that personality study is by no means coextensive with psychological research.

The writer is of the opinion that the subject matter of psychology may be classified under three headings: phenomena of behavior, phenomena of awareness,¹ and conditions of behavior and awareness. If this view is sound and if personality constitutes proper subject matter for psychological research, then personality must be classifiable under one or more of these headings.

To those psychologists who have sought personality in the phenomena of behavior,, the concept of personality has tended to assume an antiquated flavor, there being little sanction for it outside of history and popular demand. A behavioral act is something complete in itself and, surely, does not constitute personality. Some psychologists have frankly given up; others have

¹ Although the two are distinguishable, both of these categories will be referred to, hereinafter, as "behavior", the differences between behavior and awareness being irrelevant for the present paper.

attempted to rescue what they could by emphasizing recurrent patterns of behavior in the same individual. It is here that the doctrine of traits has been introduced to save the declining concept of personality, although even traits have in some extreme cases come to be regarded as merely convenient names for fictitious entities or statistical artifacts.

The recognition of traits as recurrent patterns of behavior does not, however, succeed in rescuing "personality" as a significant psychological concept. For, at any given time we have only the present behavioral acts; those past are already non-existent, those in the future do not as yet exist. The pattern of recurrence does not set aside the completeness-in-itself of each behavioral act and does not establish any intrinsic connection between the individual acts, save in our perception of them. Differently expressed, "personality," when sought in behavior, becomes not a property of the person at any given time, but a label for a group of individual acts carried out at different times, not a pattern *running through* all of his behaviors, but a pattern *composed of* his behaviors by the observing psychologist.

The difficulties of seeking personality in the behavior phenomena *per se* are enhanced by the fact that the same individual may show patterns of behavior which are by no means congruent in themselves. Thus, a child may cheat on arithmetic tests, not steal pennies, lie to mother, but tell the truth to father. It did not, however, take psychologists long, after the Hartshorne and May studies, to discern that there might very well be an actual congruence despite a superficial inconsistency in the behavior pattern. This congruence had to be sought, however, not in the behavior *per se*, but in the motives of the individual.

Here, too, psychologists have sought motives among the descriptive properties of behavior. The reason for this is not far to seek. A motive is not directly observed (save in the case of some of one's own motives, by a process of introspection), but appears in a context of motivated activity. Whatever the difficulties of observation may be, however, a motive as a logical construct loses all meaning as a property of behavior. A motive is a condition of behavior and, even though it can only be de-

ducted from the behavioral process, it still remains a condition and not the behavioral process as such.² Where introspection is possible, some introspectors may also discover the existence of motives with respect to which all activity is held in abeyance.

Allport has argued valiantly against the possibility of psychology becoming a purely nomothetic science.³ When we recognize motives as conditions of behavior, his argument loses much of its force. Scientific laws are not descriptions of phenomena, but descriptions of the conditions of phenomena. Admit the possibility of a systematic description of the conditions of psychological phenomena and you have admitted the possibility of a nomothetic science of psychology. The point is well illustrated in Allport's discussion of the Hartshorne and May data.

*Some psychologists, e.g., Koffka in his *Principles of Gestalt Psychology*, Harcourt-Brace, New York, 1935, have correctly pointed out that a description of the activities of an organism in a geographical environment is not a psychological description at all, that behavior, as such, occurs in a behavioral environment. On this conceptual level, a motive is not a property of the organism alone, but apparently also a property of the environment. It is because of this that Lewin's analysis of environmental forces can be so useful. We should not, however, lose sight of the organism as a very relevant part of the microstructure of the field which through its perceptions and motives transforms mere geographical environment into behavioral environment. Motives, even on this level of discourse, are all conditions of behavior and of the definition of the behavioral environment. Valences are only environmental reflections of motives.

³Allport, G. W., *Personality: A Psychological Interpretation*, Holt, New York, 1937. Allport appears to argue that psychology is sometimes nomothetic, sometimes idiographic, the two approaches being intertwined. "In the field of medicine, diagnosis and therapy are idiographic procedures, but both rest intimately upon a knowledge of the common factors in disease, determined by the nomothetic sciences of bacteriology and biochemistry. . . . A complete study of the individuality . . . will not be content with the discovery of laws pertaining to mind-in-general, but will seek also to understand the lawful tendencies of minds-in-particular" (*ibid.*, pp. 22-3). In so far as laws go, Allport, in common with many American psychologists, appears to confuse *general* with *common*. There are no two kinds of *law*; there are statistical generalizations and there are *laws*. As the Gestalt psychologists have long since pointed out, the individual case has to be understood in terms of laws of general validity. It is, therefore, to Allport's implication that there is *any* section of psychology that cannot be approached nomothetically that much of the following discussion is directed. It may, however, well be added that, apart from statistical generalization, Allport's argument implies that psychology as a whole can never become a nomothetic science. For there are always, if Allport is right, some determinants in the general psychological process which must remain arbitrary, so that the process as a whole remains indeterminate. This discussion is not to imply that the study of the *general* precludes the study of the *common*, but that the study of the common is but one approach to the more inclusive study of the general.

In re-evaluating these data, Allport (1, p. 25), despite his argument against the sufficiency of the nomothetic approach to psychology, appeals to conditions for which a nomothetic-scientific psychology would look. The explanation of the personal inconsistency in honesty must be sought, not in the inconsistent individuals, but in the different conditions of the behaviors. Allport mentions six conditions⁴ of behavior in this situation: bravado, mechanical interest, inferiority complex, timidity, attachment to teacher, drive for power. "Child D," he writes, "does not steal pennies, but he lies about his cheating . . . because he has a general trait of timidity (fear of consequences)." Timidity is thus a *condition* of behavior. It is not the only condition; danger of exposure is another. But given all of the conditions, we will *always* get the same results. This is a general law. Child D does not behave as Child A, not because there is no general law, but because the concrete conditions determining the behaviors are different. Allport implicitly recognizes this, although the conditions he lists are only those within the individual. It is precisely such a conditional approach that offers psychology the possibility of becoming a science, a *completely nomothetic* science.

We have already observed the difficulties associated with seeking personality in behavior *per se*. The recognition of motives as conditions of behavior sets us off on a new tack, to seek personality among the conditions of behavior. Allport's insistence that personality *does something* can only have meaning in the sense of conditioning behavior.⁵

⁴Whether these are all validly chosen, whether they are mutually disjunctive, or whether they list all of the conditions of honest-dishonest behavior is here irrelevant; our interest lies in the approach.

⁵In dealing with traits, Allport (*loc. cit.*, p. 293) writes, ". . . not only has it (the trait) become a pervading style of behavior, but also a motivational system basic in the structure of this personality." There are two distinct concepts here, and the present writer is of the opinion that it would be best to keep them distinct, the term "trait" being reserved for "a pervading style of behavior" (which we have already argued cannot be regarded as part of the structure of personality) and motives (which we shall argue are basic in the structure of personality). "A pervading style of behavior" does not *do* anything; motives *do* do something. In explaining his definition of personality, Allport (*ibid.*, pp. 48-9) writes: "Personality *is* something and *does* something. It is not synonymous with behavior or activity; least of all is it merely the impression that this activity makes on others. It is what lies *behind* specific acts and *within* the individual. The

There are three types of conditions of behavior: environmental (including, of course, the social environment), physiological, and psychological. It is obvious that if personality is to be sought among the conditions of behavior and if personality is to be regarded as something psychological, then it must be sought among the conditions of the third type. This statement should, of course, not be taken to mean that environmental and physiological factors do not condition personality. If, however, personality is one of the conditions of behavior, then environmental and physiological factors condition personality and, thereby, condition behavior in two ways: directly, and indirectly, through the rôle they play in the development and subsequent alteration of the personality which, having developed, becomes an additional conditioner of behavior. Not to minimize the complexity of the situation, we must not overlook the fact that the physiological factors which condition personality are themselves conditioned by the environment and that the organism undoubtedly plays an important rôle in conditioning the environment. We may pursue our studies as far as we like, but for the descriptive study of personality, if the term is to be psychologically meaningful, we must look to those conditions of behavior which are psychological in character.

We have already observed the potency of the concept of motivation in introducing consistency into apparently contradictory behavior. It is only natural, then, that we turn to motivation for the key to personality.

At first glance, the prospect is discouraging. Regarding the hunger motive, for example, we learn that this motive is correlated with the activity of the gastric muscle. Surely, personality is not as phasic as the activity of the stomach. Turning, however, from physiological considerations to the activity of people, we attain a better perspective. Certainly the hunger motive has *critical* phases, but we observe that behavior is often oriented toward

systems that constitute personality are in every sense *determining tendencies*, and when aroused by suitable stimuli provoke those adjustive and expressive acts by which the personality comes to be known." (Italics are Allport's.) Allport's is the merit of having courageously drawn an important distinction, but he often skirts dangerously close to obscuring this distinction.

food-getting even when the organism is not hungry. Apparently some organisms, particularly man, anticipate hunger, and this anticipation has become part and parcel of the hunger motive. The recurrent nature of certain motives and the fact of frequent frustration makes it inevitable that organisms react to motives *as motives* and develop anticipations and expectations with regard to them. Under these conditions, motives become *perpetual* beyond their critical phases. A dramatic, if somewhat fictional, illustration of how a motive-in-perpetuation may become stronger than a motive-in-crisis might be cited of a gentleman, perfectly sane, dying of thirst because he refused to drink good water and refusing to drink because of the thirst motive and because he wanted to live. This gentleman was lost in a desert, had nursed along his water supply until he had only a small quantity left. He was very thirsty, but knew what his need for water would grow increasingly acute and determined to conserve the little he had left at all costs. He finally became so weak that he was unable to avail himself of the water and died with the water still in his possession.* We begin to appreciate why psychoanalysis has sought character structure only in the "ego," for it is not in the acute phase of motives that personality will be found, nor in motives that arise only rarely and adventitiously, but in the perpetuated motives.

We cannot, however, turn our backs on the facts of physiology. Are not motives, in the last analysis, physiological in character? There is little question but that there are physiological conditions of motives. It is questionable, however, whether

*It would not be difficult to cite less fictional illustrations of the same principle, although in these cases the motivational situation is more complex. Cases have been reported of explorers and prospectors found dead of starvation, but with small quantities of food on their persons. Another case in point would be the boy who does not yield to his impulses to masturbate because he has been led to believe that this practice will diminish his sexual potency. There are, of course, many motivational factors which contribute to such a result, but the sheer pleasurable character of this activity helps to create a motive to conserve the ability to carry on such activity. The adage about having one's cake and eating it represents an ancient generalization concerning the phenomenon we are discussing. It may, incidentally, be noted that the possibility that a motive may, under special circumstances, result in behavior that is diametrically opposed to what one would ordinarily expect from this motive constitutes a strong argument for distinguishing between motives and behavior.

it is proper to speak of physiological conditions as motives at all. Stomach contraction is stomach contraction and not hunger. It is not until the restlessness occasioned by physiological process becomes canalized in some direction that a motive arises.

It is also important to realize that motives may originate which are more and more remote from the underlying physiological conditions. Thus, primarily, the hunger motive has as its object the ingestion of food, but because of conditions confronting the organism, this motive may give rise to others such as the prestige motive or the motive of workmanship. Such derived motives may also be occasional in character if the conditions giving rise to them are occasional or adventitious; but if the conditions giving rise to them are continued or recurrent, they may become perpetuated exactly as are the appetitive drives.

The whole process is complicated by the imbrication of motives into the most complex patterns. Thus, the motive to work may represent a concatenation of hunger, sex, prestige, etc., and derive sanction from each of them. The fact that derived motives may draw from many, more basic, motives implies that the derived motives may become more firmly perpetuated than the parent motives; for the combined recurrence of many motives is, of course, greater than the recurrence of any one motive. At the same time, it is probably also true that a motive, by supporting a derived motive, thereby contributes to its own perpetuation. The facts of derivation of motives and their imbrication and the fact that perpetuation of motives is, to a great extent, a matter of expectation (which may outlast its justification) give rise to an apparent functional autonomy of motives which is so strongly stressed by Allport (1, pp. 191-207).

The principle of functional autonomy of motives, however, does not bind us to the view that psychology cannot become a nomothetic science. The expression "apparent functional autonomy" was used advisedly, although it probably violates the very essence of Allport's principle. As developed here, new motives may arise and remain relatively independent of the specific motives from which they developed by becoming embodied in new motivational organizations; derived motives are never autono-

mous in the sense of standing alone; nor can one in an armchair fashion determine that a given motive is independent of its origins.⁷ Moreover, although, phenotypically, the motivational organization of each individual may be unique unto himself, this uniqueness may be a function of the uniqueness of conditions of development and not of the absence of laws of general validity. Finally, an infinite variety in the phenotype does not imply the impossibility of less varied genotypic categories subject to lawful interrelationships.

In summary, we have argued that "personality" is not to be found in behavior *per se*, nor in traits, but in those psychological conditions of behavior that we have referred to as perpetuated motives. We have further argued that the recognition of the complex organization of these motives does not preclude an approach to psychology or to the psychology of personality that is completely nomothetic.

⁷ Allport dismisses much of the psychoanalytic theory of motives simply on the grounds that it conflicts with the principle of functional autonomy (*loc. cit.*, p. 194). Similarly, Hertzman and Neff (*Conflicting Aspects of Freudian Theory*, Psychologists League Journal, 1939, 3, 30-35) speak of the "genetic fallacy" with reference to the psychoanalytic theory of motives. Yet the question is not simply one of logic nor one of a formal psychological principle. It is purely a question of fact. No doubt, anyone who maintained that *any* interest in cleanliness or in orderliness is a manifestation of anal motives would be wrong. But such an interest may, in some cases, betray, not only its derivation, but also the fact that it has not attained independence from its origins in a variety of ways: (a) through its compulsive character (b) through peculiar paradoxes of behavior (c) through a readiness to regress to the original motive under conditions of frustration. The psychoanalytic literature is replete with instances and amplifications of these points.

Allport, in a private communication, writes that the above is an exaggeration of his viewpoint. "A truer statement would be, I think, that 'Allport was driven to the theory of functional autonomy because of the absence of evidence to support the universal application of psychoanalytical theory'." In his book (*loc. cit.*, p. 194), Allport writes: "Such a theory is *obviously* opposed to psychoanalysis and to all other genetic accounts that assume inflexibility in the root purposes and drives of life. . . . The theory *declines to believe* that the energies of adult personality are infantile or archaic in nature. Motivation is *always* contemporary." (But for the italicization of "always", italics are the present writer's.) For our purposes, it is enough to recognize that Allport does not wish his views to be distorted, as is apparently easily done from the statement in his book, and that, in essence, we are in fundamental agreement with him, *viz.*, that the question involved is one of evidence rather than theory. In justice to psychoanalysis, we have but to note that psychoanalysis does not *assume* what Allport implies it does, but rests its views on evidence that may or may not be inadequate.

The typological approach to personality has, on the whole, fared even worse, in the hands of American psychologists, than has the concept of personality, meeting more often with flat opposition rather than with mere skepticism or faulty emphasis. The rationale of the opposition has generally been given in terms of the nature of the distribution of psychological variables and finds its most succinct summary in the proposition: "The type is atypical."⁸ As an argument, this proposition rests on the equivocal use of the term "typical." Literally translated, the proposition simply means that individuals belonging to certain types do not belong to the most common type.

The procedure of typing is no more and no less than a procedure of classification. It makes no further assumptions than that for a given purpose or for certain operations, and with a certain margin of error, case *A* is equivalent to case *B*. If the normality of distribution is relevant, it is only in determining the percentages of a population that fall into given classes. If the continuity of distribution is relevant, it is only in determining that some cases in a given class may be substituted for some cases in another class without much more error than would ensue from substituting certain cases in one class for certain other cases in the same class. Where typing proceeds on the basis of some mensurational procedure which is itself subject to error, the fact of continuity may contribute to the classification of a given case under one category when it properly belongs in another category. Thus, a given individual may, as a consequence of error of measurement, be classed as a moron although he is properly an imbecile. The error is in the diagnostic procedure, however, not in the diagnostic categories, although it is of course true that the nature of the categories will condition the possibilities of error in actual measurement.

So far as the system of classification is concerned, the relevant questions have to do with the objectives of the system and the feasibility of putting the scheme into practice. Continuity

⁸ Cf. Woodworth, R. S., *Psychology*, 4th edition, Holt, New York, 1940, pp. 66-67: "The typical individual is the one of the so-called mixed type, while the rare individuals taken to represent the pure visual or auditory type are really *atypical*" (emphasis is Woodworth's).

of distributions and errors of measurement do affect the feasibility of any method of classification in which they are involved. Yet, the difficulties introduced need not necessarily counterbalance the need for classification, nor the advantages derived therefrom. For classification does serve an extremely important function for science, the function of economy. If there are certain other dangers in the typological approach, such as that of forgetting that an individual who has been "typed" is still an individual and not a type, or of assuming that once an individual has been "typed" he is no longer subject to change, these dangers inhere in the ineptness of the typologist rather than in the typological approach.

In approaching personality from the viewpoint of typology, certain distinctions should be made. One may seek to type persons, or behaviors, or personalities; the three kinds of typology have been too often confused under the heading of typology of personality.

The typing of persons is, properly speaking, not a task of psychology at all. Psychology is concerned with behavior and its conditions, not with persons. It is true that only organisms behave and that we are apt to be most interested in a particular organism, man. But so are other sciences interested in man, who represents a point of convergence of many sciences. Granted a primary interest in man, each individual science can make its best contribution to this study by not confusing its subject matter with its ultimate objective.⁹

The typology of persons has been approached from two different angles: on the other hand, a pure typology of persons based on inverted factor analysis or some procedure such as that outlined by Lazarsfeld¹⁰ and, on the other hand, a typology of persons resting on a typology of behavior. In this latter typology, the principle of classification involves the predominance in persons of a given type of behavior. Thus, introversion and extra-

⁹ The typing of persons does not, of course, proceed in the abstract and when engaged in by psychologists is carried out with reference to some psychological variable. Such a procedure is perfectly legitimate, provided that it does not obscure the fact that the classification fundamentally pertains to the psychological variable.

version refer primarily to two types of behavior, classified from the point of view of inward or outward orientation. Person-types depend on the degree to which subject shows a preponderance of one or the other type of behavior. In the same way, one might classify persons as adient, abient, or ambient, depending on the relative balance of behaviors of approach or withdrawal, or as active or passive, depending on the habitual involvement of the musculature.

Typology of behavior is undoubtedly relevant to psychological research, and often extremely valuable. It does not, however, fulfill the functions of a typology of personality. Behavior is not only conditioned by personality, but also by environmental factors. The typology of behavior does not get at these diverse conditions. Suppose, for example, that introversion-extraversion is determined by the need of security, introversion being favored by insecurity, extraversion by security. If this were the sole determination, then a given individual would be introverted in a dangerous environment, extraverted in a safe environment. He would be habitually introverted only if he were habitually in a dangerous environment.

The confusion of a typology of persons based on a typology of behavior with a genuine typology of personality has been abetted to the extent that individuals in the same social stratum tend, on the whole and in the long run, to face the same environment. In so far as this sameness of environment is maintained, there is, of course, high correlation between differences of customary behavior and differences of personality between individuals. The constancy of environment is not, however, too dependable.¹¹ To save ourselves confusion in a welter of seeming paradoxes, it is necessary to probe deeper than the mere typing of behavior; it is necessary to recognize that the same personality structure, whether it be in the same or in different individuals,

¹¹ Lazarsfeld, P. F., *Some Remarks on the Typological Procedures in Social Research*, *Zeitschrift für Sozialforschung*, 1937, pp. 119-139. This paper deals with methodological aspects of typology. Reference to it in the present context is intended merely to indicate that, particularly when the number of variables is small, Lazarsfeld's procedure offers a simple approach to the typing of persons. Its utility is not limited, however, to this purpose.

may lend itself to diametrically opposed behavioral manifestations and that different personalities may lend themselves to the same behavioral manifestation, depending on the circumstances in which the individual finds himself.

A typology of personality seems to be called for, and we propose, here, to consider some of the criteria in terms of which such a typology may be evaluated. First, we must recognize that a typology, being a *typology*, must be constructed in terms of genotypic, rather than phenotypic, concepts. Beyond that, we shall propose three criteria: that the variables of the system be concerned with internal psychological conditions of behavior, that the system be universally applicable, and that the system be unequivocal in structure.

The first of these criteria stems, of course, from the considerations we have raised concerning the nature of personality. The second arises from a demand for a nomothetic science of psychology. As each individual personality is described in more and more unique terms, the possibility of a systematic nomothetic description becomes more and more difficult¹² of attainment.

¹¹ We have been talking in terms of geographical environment. In referring to "dangerous" and "safe" environments, for example, we have pretended that danger and safety inhere solely in the environment. Of course, they do not, being to a large extent defined by the organism itself. Where the constancy of geographical environment enables us to arrive at differences between individuals which correspond to their different personalities, it is only under the condition that behavioral environment corresponds to geographical environment. Unfortunately, we cannot judge the constancy of the behavioral environment from that of the geographical environment. There is no doubt but that the description of behavior in terms of behavioral environment would solve many of our problems, but we have already seen that the analysis of the motives of the individual is logically prior to speaking of behavioral environment at all. The typology of behavior, as an approach to personality, is caught between the relative barrenness of the geographical environment for purposes of psychological understanding and the need of first knowing the individual whose behavioral environment is to be defined.

¹² Difficult, rather than impossible, because the laws which psychology seeks are primarily *laws of behavior*. If personality conditions behavior, the larger the number of described personalities, the greater the number of laws necessary to nomothetic description. The alternative to universality is, however, and fortunately so, obviously not uniqueness. The task of psychology is to devise genotypic concepts which embrace larger and larger numbers of phenotypes and which may, at the same time, be embodied in lawful generalizations. Universality is the ideal

Universal applicability requires that it be possible to assign to each personality its status with respect to each variable of the system, whether the status is described simply in terms of "has" — "has not" or in more precise quantitative terms. The various psychoanalytical categories of character structure, for example, in terms of oral, anal, genital, etc., needs, do not appear at present to have universal applicability in the sense we have indicated. An individual may be described as *either* oral, or anal, or genital, or some combination of these; each personality is described with reference to some segment of the system rather than with reference to the system as a whole. This characterology, at the present time, represents a series of group-systems rather than a universal system; but it can attain universality through a more rigorous definition of oral-related, anal-related, genital-related, etc., needs and through an evaluation of each personality with respect to all of these. This may transcend the practical requirements of psychoanalysis, but is necessary for a systematic psychology of personality.

The third criterion, unequivocally, stems from the increasingly evident fact that the same characteristic may play entirely different rôles in different personality structures. Traditionally, where this problem has been faced at all, the approach has been to seek to determine the unique structure of the individual personality, an approach which in practice abandoned the nomothetic-scientific goal. To ignore the fact, is to sterilize psychology. To accept the fact and the traditional approach to it, is to abandon the goal of making psychology a nomothetic science. For, although one may find a set of concepts which are applicable to all people, if these concepts are uniquely applied to each individual, there can be no law. Scientific law must account for the individual case, but must do so in an orderly fashion. The solution is to eliminate the equivocality from the *system* to which personalities are being ordered; the variables of the system must

to which we must strive. Whether it can be attained, the future must decide. It may, however, be that the best psychology can attain will be a number of systems each applicable to a large number of personalities, or a universal system which requires supplementation by group-systems.

themselves be unambiguous and must allow no equivocal inter-relationships.

The project we have outlined is an ambitious one, representing rather a hope for the future than a prospect of immediate attainment. Yet, in prospect, at least, it does not appear to be impossible of attainment. The most difficult of the tasks confronting us is the discovery of a set of unequivocal variables that can do justice to the richness of the human personality. One suggestion, and it is hardly more than a suggestion, comes from the very discovery of the equivocality of certain variables. If introversion is equivocal because, in one person, it rests on a need for security, while in another it rests on the inability to bear frustration and the lesser likelihood of frustration in phantasy, while in a third person, it rests upon sheer richness of creative inner life which because of its very richness finds no sufficient avenue of expression, then it is possible that in such new variables we have less ambiguous variables than introversion and variables whose interrelationships are less equivocal. It may be that through rigorous systematic analysis of the ambiguities that do arise, we shall discover unequivocal variables. But, however it is done, the discovery of such variables remains one of the central tasks of psychology today.

We shall now attempt to clarify and amplify some of the previous discussion by specific consideration of a few systems of personality typing that have been proposed. Limitations of space, of course, prevent any attempt at an exhaustive treatment.

The most familiar typology is, of course, that of introversion-extraversion. Many psychologists have been disturbed by this system, as a typology, because of the continuity and the normality of the distribution, but we have already seen that this disturbance is over a matter that is not at all critical for a typological approach. More significant is the discovery of the multidimensionality of introversion-extraversion. It may be well, at this point, to recall that this discovery does not at all conflict, in spirit, with the original theory. Jung's secondary typology of "thinking," "feeling," "intuition," and "sensation" types amounts to a multidimensional theory of introversion-extraversion. The

fact of multidimensionality, however, only means that the system must be more complex than was at first supposed.

Space does not permit a detailed examination of the individual versions of introversion-extraversion theory, nor is this necessary for our immediate purpose. We have already pointed out that introversion-extraversion refers primarily to a typology of behavior rather than a typology of personality. It would, however, appear simple to convert it into a typology of personality by postulating the existence of needs that correspond to these behaviors. In arriving at a diagnosis of a given individual, on this basis, it would be insufficient merely to use a check list of indicators, but it would be necessary to make a careful analysis of the relation of the individual to his environment, not in terms of what he does, but in terms of what activates what he does.

Such a procedure might result in a typology of personality that is universally applicable, but it would still be markedly deficient when evaluated in terms of the criterion of unequivocality. Apart from the intrinsic equivocal nature of the variable (introversion-extraversion) to which we have already had occasion to refer, there is an equivocal element that enters from another direction. It is highly doubtful that any univariate typology of personality can do justice to the material it seeks to classify. This implies that introversion-extraversion would have to become a component of a larger system into which it can unambiguously fit, a system all of whose remaining variables are unequivocally interrelated. Such a *system* has never been attempted, although introversion-extraversion has, of course, been included in larger lists of personality variables.

In apparent contrast to the univariate typology of introversion-extraversion, stands the seemingly multivariate typology advocated by Spranger. Yet, the difference in this respect is more a matter of linguistic emphasis than a genuine difference. We, more often than not, speak of introversion-extraversion as a single dimension of personality even though we recognize that it is a complex dimension. But the six dimensions of Spranger's system, after all, refer only to a segment of the total personality,

namely, its values. Again we find the need of incorporating Spranger's system into a more elaborate system, with all of the ensuing difficulties.

In one respect, the typology of values is formally superior to the typology of introversion-extraversion. It unambiguously deals with needs rather than with behavior. Psychologists who seek motives among the attributes of behavior have, of course, reintroduced the ambiguity. Thus, Allport and Vernon, in their "Study of Values," do not distinguish between interests and values. Yet interests are primarily modes of behavior (albeit they imply needs), behavior of approach or withdrawal, and values are primarily needs. Recognizing this fact and the fact that behavior may appear contradictory to the motives from which it stems, we can understand reactions of self-depreciation or of self-condemnation to unworthy interests without assuming that the values with which they conflict are not genuine. A typological system based on values deals with *values*, not with interests, and the separation of the two would enhance the study of both.

The chief difficulty with Spranger's system is that the needs with which it deals are so completely equivocal and are so far removed from the basic needs which activate the organism. The value "power" may, for example, stem from a need of self-assertion or its direct opposite, self-abasement; or, it may stem from a need directed toward a loved or hated *object* by, for example, maintaining the object on, or removing it from, a pedestal. The child whose world was shaken to its very foundation by the discovery that his father is not omnipotent, may, in his value of power-above-all-else, be seeking to restore the security of the foundation.

There have, of course, been many approaches to personality couched in more basic dynamic terms than those mentioned above. Somehow, however, the dynamic approach in terms of needs has escaped the typologists. We need but mention the relatively recent monumental work of Murray (5) and his co-workers as an instance of such dynamic approach. Yet, the elaborate system which Murray has erected is not readily assimilated, in its present form, to the typological approach, partly because of its fail-

ure to satisfy the criteria we have suggested and partly because of the very multiplicity of the concepts involved.¹³ Murray was not, of course, intending to establish a typology, but was primarily oriented toward the individual as a phenotypic unit.

A similar preoccupation with the individual as a phenotypic unit has, in general, characterized the psychoanalytic approach, although several psychoanalysts have sallied into the typological field. Thus, various character types, such as the oral, anal, genital, neurotic, homosexual, etc., characters have been described. We have already had occasion to refer to the deficiencies of this approach, from our point of view. It remains to be added that this characterology deals with only a segment of the total personality, being, for the most part, concerned with the structure of the "ego."

Freud (2) has, however, described a typology of the total personality and it is one which comes closer to satisfying the criteria we have suggested than any of the other typologies, although it also raises many new problems. The system consists of three basic types and four mixed types. The basic types represent the dominance of id, ego, and superego, respectively.¹⁴ The

¹³ Mere multiplicity of concepts is not objectionable; unordered multiplicity is. There is no attempt to distinguish the hierarchy of concepts that is involved in the transition from more genotypic to less genotypic and, even, phenotypic concepts. There is no systematic attempt to determine the interrelationships of the thirty-one needs that are listed. There are also other sources of confusion. Presses, for example, are, of course, relevant to the study of personality, but they are only relevant as conditions of personality development and of the operation of the personality, unless they have become internalized and thereby ceased to be *presses*. Failure to make such distinctions operates only to confuse our understanding of the nature of personality.

¹⁴ Freud does not, in this paper, define these concepts. The following definitions may, however, be suggested as conforming to Freud's argument and to general psychoanalytic usage:

Id motives are motives low in the hierarchy of motivational derivations (being either appetitive drives or close to appetitive drives) which press for realization without regard for consequences.

Ego motives are motives stemming from the barriers and presses imposed by outer reality (the barriers and presses being experienced as such by the individual) and directed towards the maintenance of a maximum degree of gratification of *all* of the individual's motives. The existence of ego motives is conditioned, on the one hand, by the existence of id and, later, superego motives and, on the other hand, by the perception of barriers and presses imposed by the outer environment. The "ego" is a term which is used in psychoanalysis to include the ego motives and the perceptual and motor-control mechanisms.

mixed types represent the various combinations of the basic types. Thus, the id-ego type represents a relatively equal balance of id and ego motives, superego motives being relatively weak.¹⁵

Such a typology is based on a genotypic classification of all of the organism's motives. As a typology based on motives, rather than on behavior, it fulfills our first criterion, being con-

Superego motives are the internalized quasi-barriers (inhibitions) and quasi-presses which, having become internalized, function as motives rather than as barriers or presses and which also function without regard to the actual outer presses and barriers. We speak of *quasi*-barriers and *quasi*-presses because the individual does not experience their justification in fact. An external prohibition, carrying with it the threat of punishment, is a barrier and the need to conform to it is an ego motive. If the threat of punishment is removed, and the individual is in a position to detect its removal, but the prohibition still functions, the prohibition represents a quasi-barrier.

Motives may, of course, occur in complex form, often, if not always, combining into a single motivational complex id motives, ego motives, and superego motives. The system Freud here suggests involves breaking down such motives into their id, ego, and superego components. It is well to remember that the process of rationalization (an ego function) may conceal the id and superego components under the guise of ego motives. The need to sustain the rationalization is, of course, an ego motive so that motives arrived at through a process of rationalization may become genuine derived motives.

¹⁵ Freud's verbal usage in this paper is strikingly inconsistent with his general psychology. The term "libidinal types" for this typology is peculiarly inappropriate and makes for a faulty emphasis. He suggests the following names for the basic types: erotic (id), narcissistic (ego), and compulsive (superego). Each of these names is faulty and hence we have not used them. Erotic motives are by no means co-extensive with id motives, even by his own usage; witness the aggressive motives. Narcissism applies to the ego only in an infantile form and can hardly serve as a generic description of ego motives. The term "compulsive" for superego, while it does serve to emphasize the external origin of the superego motives, stems from the role the superego plays in the compulsional-obsessional neuroses. The compulsional symptom may, however, express a relative victory of an id motive, witness the compulsive masturbatory gesture.

Freud also speaks of the id-ego-superego type as the ideal type, in the sense of the perfectly adjusted type, because of the perfect balance of the three groups of motives; and, incidentally, probably also representing a null-class. This, too, is inconsistent with his general outlook, and probably arises from a temporary confusion of the ego with the infantile ego. Superego motives are undoubtedly useful at times in preventing the individual from undertaking activities which entail harmful consequences. Such occasions involving usefulness of superego motives arise because of the relative inadequacy of the ego, particularly in its perceptual functions. Because of their inadequate relation to reality and to id ego motives, however, superego motives represent a mighty unreliable safeguard. From Freud's point of view, the ideal personality should be of the ego type, provided the ego is mature and highly developed (*i.e.*, provided the scope of the individual is very broad, his intellectual functions highly developed, his insight, self-control, ability to bear pain and frustration maximized, etc.).

cerned with the internal psychological conditions of behavior.¹⁶ This typology is also acceptable from the viewpoint of the second criterion, universality, although its range of applicability may be limited to humans. The typology is not culturally limited since it has no cultural content, although it is conceivable that there may be cultures where one or more of these types may be null-classes. The typology also satisfies the third criterion, unequivocally, for the interrelationships between id, ego, and superego motives are defined implicitly in the system of classification and the variables themselves are subject to rigid and unequivocal definition.

The satisfaction of the criteria we have proposed by this typological system does not, however, leave us without any difficulties. There are still a number of problems that are raised by this system.

First, suppose that we have classified a given personality under this system, how much does the classification tell us? It obviously does not tell us whether we are dealing, in a given personality, with erotic or aggressive id impulses, with mature or immature ego motives, and so on. But this is precisely characteristic of any genotypic system of description, which does not distinguish between items included within a given category. As such distinctions are added, we pass from genotype to more and more phenotypic description. Thus, to the id type we may subsume the subtypes of erotic or aggressive; and to the erotic subtype, still further subtypes of oral, anal, genital, etc. (not to be

¹⁶ It will be noted that we felt that personality has primarily to do with perpetuated motives. Such motives belong only to the ego and superego systems (cf. the psychoanalytic term "character"). While id motives are probably never perpetuated (when the perpetuation process occurs, it becomes part of the ego structure, the motive-in-crisis retaining its id character), they do have recurrent patterns and a common character (cf. the definition of id motives) which makes it possible to classify them in one group. The individual develops ways of dealing with motives of this group, either permitting them expression (id dominance) or curbing them (ego or superego dominance) or giving them only partial or indirect or deferred satisfaction (relative dominance of the id or of the ego or of the superego). Such patterns genuinely pertain to the structure of the personality. Even if we restricted the concept of personality to systems of perpetuated motives, the id personality would be a meaningful concept (*i.e.*, as a personality in which the ego and superego systems are not well developed).

confused with similarly named ego types), or of autoerotic, homoerotic, etc. types.¹⁷

As we pass along this hierarchy of classes, the class names become less and less universally applicable (*i.e.*, they become applicable to smaller and smaller groups until the ultimate phenotypic unit, the individual personality, is reached). Needless to say, somewhere along this hierarchy, the cultural limitation sets in.

This brings us to our second problem. Once we begin to speak of the subtypes (and we must remember that we are classifying motives, not persons), we begin to wonder whether a given individual may not be id dominated in one respect, ego dominated in another, and superego dominated in a third. Then, again, we wonder whether a given individual may not be id dominated in one situation, ego dominated in a second, superego dominated in a third. That is to say, we wonder about the consistency of the patterns and, in terms of our experience, can only anticipate that there is no consistency.

In the face of this difficulty (and it is a difficulty because, although our subject matter may be behavior, our ultimate interests reside in individuals), most psychologists would undoubtedly be tempted to throw the whole system into the scrap heap. Yet, there may be a way out. It is necessary to determine the important areas of the individual life space and to define the personality within these areas. If, for one individual, love is the central thema of his existence, then it is within the area defined by this thema that the basic picture of the personality will be found. Another person may present self-assertion as the central thema and, yet, within this area have the same personality structure that the first has in the area of love. Within this area, there may be no contradictions, and it is even possible that in relation to the personality configuration presented in this central area all other areas may acquire meaning. That is, a given in-

¹⁷ The above illustrations still follow orthodox psychoanalytic theory. For the sake of clarity, it is well to point out that the use of the concepts of id, ego, and super-ego in any of its versions. It may be well also to caution against the confusion of "subtypes" referred to above with "mixed types" referred to earlier.

dividual may have many personalities, one of them being central and, perhaps, explaining the others. The typing of persons could be carried out in terms of these central areas and, for many purposes, the differences in the locus of the areas may be irrelevant.

All of which brings us to our third problem. It is one thing formally to define id, ego, and superego; it is another to decide in practice to which category a given motive belongs, or, in the case of a complex motive, which aspect predominates. It is one thing to speak glibly of the central themata of an individual's psychological existence; it is quite another thing to determine what these themata might be. The problem of diagnosis is critical to this system and, at the present time, there is no royal road to such diagnosis. The difficulty is inherent in any attempt to diagnose motives, for motives are never given directly, but must be inferred from behavior. The form of experimental test is clear enough: barriers to the assumed goals must be introduced or removed, and hypothecated goal objects or situations altered. In practice, however, when motives are at all complex, such experiments are not easily carried through. One must at present depend largely upon clinical intuitions arrived at from masses of information about the individual. Yet, if such intuitions are more than mere guesses, it must be possible to refine the intuitional process to the point where they may become clearly expressed principles of judgment.

Finally, assuming that all of these difficulties are overcome, would the resulting system be worthwhile from the point of view of facilitating the nomothetic approach to psychology? It is impossible to answer this question as yet, for the simple reason that no one has attempted to work with this system. Psychoanalysts in practice seem to be much too preoccupied with the individual case to find useful such broad genotypic categories of personality. Yet, psychoanalysts in practice are, at present, in by far the best situation to arrive at the necessary diagnoses. Whether for this or for some other system, there is a genuine need for a marriage of clinicians and experimental psychologists in order to further the aims of a nomothetic science of psychology.

The difficulties with Freud's typology are far too great for it to meet with our unqualified endorsement, and its discussion is not to be interpreted in the sense of "Here is the solution to our problems." The space devoted to it is only tribute to a system which comes closest to filling our needs and, if it is still found faulty, it is but an index of how far we have yet to go. It does not matter whether we finally retain this particular typology. It does matter that students of the psychology of personality occupy themselves with the study of systems of motives. If the study of motives offers difficulties, it is of no avail to ignore the difficulties by declaring that there are no motives, that there is only behavior. Difficulties of exploration do not justify the ignoring of a field of research.

SUMMARY

We have argued for a nomothetic-scientific approach to psychology; that personality is best viewed among the conditions of behavior; that the structure of personality consists primarily of systems of perpetuated motives; that traditional objections to the typological approach are not valid; that the typological approach is both useful and feasible. We have suggested several criteria in terms of which to evaluate typological systems. Having applied these criteria to a number of typologies, we found the system suggested by Freud in terms of id, ego, superego, and mixed types to come closest to satisfying the criteria. Because of many secondary problems which arise in connection with Freud's typology, we did not endorse it, although we felt that it strongly merits further exploration. We do derive from its discussion the encouragement that the kind of approach we are advocating is possible.

REFERENCES

1. ALLPORT, G. W. *Personality: A Psychological Interpretation*. New York: Holt, 1937.
2. FREUD, S. Libidinal types. *Psycholanal. Quar.*, 1932, 1, 3-6.
3. HERTZMAN, M., & NEFF, W. S. Conflicting aspects of Freudian theory. *Psychol. League J.*, 1939, 3, 30-35.
4. LAZARSFELD, P. F. Some remarks on the typological procedures in social research. *Z. f. Sozialforsch.*, 1937, pp. 119-139.
5. MURRAY, H. A., *et al.* *Explorations in Personality*. New York: Oxford Univ. Press, 1938.
6. WOODWORTH, R. S. *Psychology*. (4th ed.) New York: Holt, 1940.

PART II

SOCIAL PSYCHOLOGY

The rapid increase in relative importance of the specialty of social psychology within the family of all psychological disciplines has been conspicuous since about 1930. One outstanding feature of its recent development was the founding of the Society for the Psychological Study of Social Issues in 1936 — an event highly symptomatic of the character of the extraordinary cultural crisis now affecting men everywhere. Since 1940, the demands of a war government have shunted many social psychologists into confidential research and service projects that cannot be effectively evaluated until something like normal perspectives are again possible.

In the papers selected for representation in this section, an attempt has been made to group such fresh studies as faithfully represent the current scene without being wholly immersed in it. The basis of choice has been simply this: What set of articles may be collected which will readably portray (1) the range of interests, (2) the different methods, and (3) the varied content of social psychology in America today? This procedure yields a miniature anthology quite distinct from the attempt to assemble the very best technical studies as such.

If the reader can succeed in overcoming his initial inertia to the perusal of the first paragraphs of these diversified reports, he may be assured that both entertainment and enlightenment will be his rewards.

GEORGE W. HARTMANN

IDENTIFICATION AND THE POST-WAR WORLD ¹

EDWARD CHACE TOLMAN

University of California

One psychological process which seems to me to need especial emphasis in planning a post-war world is "identification." Identification was apparently first noted and named by Freud. But his conception became unnecessarily complicated and it was too closely bound up with his whole psychoanalytical system.² I shall not here mean by identification, therefore, Freud's own concept, but merely a certain general neo-Freudian notion which seems now to be widely accepted by most psychologists and sociologists.³ Examining further this neo-Freudian notion we find that it really covers three somewhat different, though related, processes.

First, by identification may be meant the process wherein an individual tries to copy — to take as his pattern or model — some other older (or in some other way looked-up-to or envied) individual. This tendency is, of course, especially observable in children. It is the form of the process with which Freud was most concerned. He saw it primarily in the attempt of the small boy to mold himself after his father. The boy wishes to copy the father and (according to Freud) to replace the latter in the

¹The author regards the present contribution as an extension of the argument contained in his *Drives toward war*.

²See, for example, the presentation of Freud's doctrine in Healey, Bronner, and Bowers, *The structure and meaning of psychoanalysis*. New York: Knopf, 1930. Pp. 240-247.

³Many who accept this general notion would, indeed, violently deny any direct Freudian affiliations.

mother's affections. Or the girl tries to take on the pattern of the mother. Children thus tend to identify with the parent of the same sex. And later identifications are said to follow and symbolize these earlier ones. We as adults have heroes, social and political leaders, movie stars, and the like, with whom we identify in a similar way to that in which as children we identified with our parents.⁴

A second process also meant by identification is the adherence of the individual to any group of which he feels himself a part. This, according to Freud, is a symbolic repetition of the feeling of the child for his siblings. But, in any case, it is a very real phenomenon with which all sociologists are familiar. It underlies patriotism, and mob action, the Oxford Group movement, and the activities of the Townsend Clubs.⁵ And in a recent study of a simple village, it appeared that identifications with one's family groups, with one's co-religionists, and with one's fellow villagers were all very important features of life in the community and appeared most clearly in "such indexes as proverbs, swearing expressions, names, addressing others, marriage, and patterns of conflict and co-operation."⁶

Finally, the third process also usually called identification is the acceptance by an individual of a cause. One accepts and gives oneself not only to groups, but also to seemingly quite impersonal causes such as the progress of science, temperance, public health, wearing the right clothes, internationalism, the abolition of war, etc. Such cause-identifications are legion; and they vary from the sublime to the ridiculous. It is my belief, however, that they are, in the last analysis, but expressions of what were initially and more fundamentally group-identifications. The essence of a group-identification is, I would hold, the fact that one desires "to love" and "to be loved" by some group. And such desires lead inevitably to one's adopting the values and causes proclaimed by the group.

⁴ It must be noted that older individuals can also identify with younger. For not only do children identify with their parents but, *vice versa*, parents often identify with (*i.e.*, live vicariously through) their children.

To sum up, there are three interrelated kinds of identification: (1) that of an individual with some other older and more important (or in some other way envied and preferred) person whom the individual in question wants to be like; (2) that of an individual with some whole group which he wants "to love" and "to be loved by"; and, finally, (3) that of an individual with a cause proclaimed by a group.⁷

II

I wish to consider, now, the second of these processes — that of group-identification — more fully. For it is the one we shall be primarily concerned with in the present argument. It appears to be a most powerful propensity. Every one of us here in America tends to identify, in differing degrees, with such groups as his family, the other members of his socio-economic class, his occupational or professional colleagues, the co-residents of his municipality, the citizens of his state (especially if the latter be California), the citizens of the USA, the peoples of the Western Hemisphere, the English-speaking peoples, and the like.

And, insofar as one does thus identify, he tends to feel at one with each such group. *Its* fortunes are *his* fortunes; *its* goals become *his* goals; *its* successes and failures, *his* successes and failures; and *its* prestige becomes *his* prestige. And, in the extreme case, the continued life and immortality of such a group comes to be felt to be the equivalent of and a substitute for his own personal life and immortality. A man will die in order that his country may live. The latter's life comes in some mystical way to be identical with and a substitute for his own personal life.

⁷ See, for example, Hadley Cantril, *The psychology of social movements*. New York: Wiley, 1941. Cantril implies, though he does not explicitly state, this relationship between identification and group action.

⁸ Afif I Tannous. Group behavior in the village community of Lebannon. *Amer. J. Sociol.*, 1942, 48, 231-239.

⁹ It may be pointed out that identification with groups and with causes, as I am here conceiving them, are undoubtedly closely related to what social psychologists are now calling 'morale.' See, for example, Goodwin Watson, *et al.*, *Civilian morale* Boston: Houghton Mifflin, 1942, especially the definition of morale given by G. W. Allport, *op. cit.*, p. 5.

III

But, we must ask, what are the characteristics which make a given group a strong evoker of identifications? Five such features or characters suggest themselves:

1. The possession by the group of common characteristics which clearly set off the group members from nonmembers will favor identification. If the members all have a common language, a given skin color, a particular name (*e.g.*, Greeks, Romans, Americans), a particular history (*e.g.*, North America, the Western Hemisphere), a common history, and the like, identifications will be enhanced.

2. Distinctive symbols and rituals which belong to the group, such as a flag, a song, a fraternity pin, fraternal rites, and the like, will also all help to make identification easy. During a war much use is, of course, made of such symbols and rituals. We rise and sing the national anthem and salute the flag, thereby enhancing our patriotism, *i.e.*, our identification with the national group.

3. A common goal animating the group and giving it a feeling of mission will increase the readiness to identify. Thus "block organizations" today are groups whose members are coming to identify to a surprising degree simply because such block organizations have the obvious and common goal of protection against air-raids. Or, to take another example, the Nazis have been tremendously strengthened in their identifications with the party by being given the common mission to spread the master race over Europe.

4. A set-up in the group which capitalizes and symbolizes the early family relationship and structure will strengthen identifications. Japanese patriotism (perhaps even more than other patriotisms) employs this device. Gunther quotes the following from the Enthronement Edition of the *Japan Advertiser*:

Not only is the Shrine of Ise a holy spot in the religious sense, but it is the visible symbol of the nation's whole being. The Japanese attitude toward it is one of *makoto*, a word which cannot be accurately rendered into English. Patriotism, nationalism, Emperor worship, the attitude toward the throne, are words or phrases used for *makoto*, but each of

them is very inexact. Loyalty, filial piety, the emphasis on the family rather than on the individual, are still other attempts to put *makoto* in English. *Makoto* embraces all these, but no one of them has the exact connotation to the Japanese consciousness that it has to the American or European. Foreign thought does not comprehend the reverence, loving loyalty, respectful *kinship* of the Japanese toward his Emperor, and therefore toward the nation, and therefore toward himself as a part of the nation.”⁸

Here, it is obvious that the nation is patterned after and symbolizes the family.

5. Identifications are strengthened by common enemies. Thus Jews to the Nazis, or Negroes to a lynching mob, or capitalists to the communists, or communists to the solid business man, perform a tremendous function in enhancing intragroup unity.

IV

We turn, finally, to our main problem — identification and the post-war world. For, insofar as the human beings in the world are distributed into (and identify with) geographically separate, mutually exclusive, political groups and insofar as such groups compete for the same goals — viz., territorial rights, natural resources, and the like — there are bound to be wars. The only conceivable way in which wars can be overcome is through the development of widespread identifications with some all-common supranational group. Only if individuals belonging to every tribe (or to every modern surrogate for a tribe, a nation) can come to identify, not merely with their own tribal groups as such, but also (and more powerfully) with such an all-inclusive super-tribe or World State can wars be abolished.

Furthermore, such a World State or World Federation will operate only if (in addition to appropriate and wise political structures) this State or Federation be lent psychologically requisite, identification-evoking characteristics. It must be provided with all the necessary psychological *accoutrements and trappings*.

⁸ John Gunther. *Inside Asia*. New York: Harper, 1939, p. 7. Primitive tribes also capitalize on the same sort of structure in their concept of the ancestral totem animal.

It must be made tremendously appealing. Or, to return to our list of identification-evoking characteristics suggested above, this means: (1) that the members of the World State should possess features in common setting them off from nonmembers; (2) that the World State should have distinctive symbols and rituals; (3) that there should be a common mission animating its members; (4) that its governmental structure should symbolize early family relationships; and finally (5) that it should have some outside enemy or enemies which threaten it. Let us consider each of these in more detail.

1. *The Possession of Common Features.* The members of the World State must be given common features which set them off not from individuals outside of the world (for we know of none such) but from themselves in their own narrower roles as members of mere subordinate groups. I can think of two types of such setting-off features:

a. There could be a common world language. This would not supplant the narrower national tongues but would be an auxiliary to them. Individuals all over the world would be required to learn it in addition to their own separatistic tongues.⁹

b. There could be a common basic education. I would envisage, that is, the setting-up all over the world of special classes (for children and for adults) in which the official world-language and the basic facts of the world constitution and of world problems such as raw materials, trade, public health, disease, population pressures, and the like would be taught and discussed. Such an educational set-up should be an integral part of the peace covenant. Such a common language and such a common education would constantly emphasize our common human similarities in contrast to our subordinate separatenesses of skin-color, race, geographical location, and the like.

2. *Distinctive Symbols and Rituals.* There should be a world flag and a world anthem. The children and adults in the

⁹ For a most impressive discussion of this need for a world language and the problems involved in selecting such a language see Albert Guérard, *The France of tomorrow*. Cambridge: Harvard Univ. Press, 1942, Chap. XII. Also Albert Guérard, International language and national cultures. *Amer. Scholar*, 1941, 10, 170-183.

world classes should salute their world flag and sing their world anthem as we now arise and salute our national flag and sing our national anthems. There must also be a common currency and common postage. For (aside from their economic benefits) such a common currency and common postage would have enormous symbolic and ritualistic values. If on every coin and on every stamp we saw not, as now, a narrow tribal symbol and a narrow tribal motto but a world symbol and a world motto, it certainly would help all of us to identify with the larger world whole.

3. *A Common Animating Goal or Mission.* Such a common goal or mission for the World State might consist of two main features:

a. First, there could be the universal aim of no further wars. There are undoubtedly films documenting the present war which could be constantly shown to keep alive the horrors, the complete senselessness, and the destructiveness of modern war. Such films must be shown again and again in our world classes. They might succeed easily in building up a feeling of common aim or mission.

b. A second aim could be the more even distribution of the world's goods to all peoples and to all individuals. This aim could be continuously discussed in the world classes. But this, it may be said, is postulating too Utopian an impossibility. Individuals and groups, it will be contended, are too selfish. They would rather continue to have wars, no matter how horrible, than give up any of their selfish privileges. And this, of course, may be true. But if so, there is no hope and we may as well stop talking. But the presupposition of the present argument, and indeed of this whole Symposium, is that there *is* (or *could be*) a large enough or powerful enough proportion of the world's population who are not too selfish, who are willing to pay the price, and who would be able to put over such a "world new deal."

4. *A Set-up Which Symbolizes Family Relationships.* This requirement suggests to me that the international government should have—in addition to a Secretariat, a Legislature, and a Court—an Executive Head (individual or committee). For such a head would symbolize a father (or group of fathers) to whom

all individuals could feel loyal. If this executive head were a committee, then it could comprise one European, one Middle Easterner, one Asiatic, one African, one American, etc. Or, if the world as a whole is to be divided into a series of component federations, as many post-war planners have been suggesting, then such a Committee could have one representative (father) from each of such component federations.

Secondly, the World Government must have some well-located geographical seat, which will show up bright and important on the map. And the buildings of the world government, located at this seat, should be of unexcelled grandeur. They should symbolize one's idealized home. There should be pictures of them in every hut and hovel. They should be "archetypal" in character. And there should be movies depicting the governing bodies "carrying on" in this "home" surrounded by delightful pomp and circumstance.

5. *Common Enemies.* This requisite would seem the most difficult to encompass. If only the men from Mars were a reality and if only these Martians would attack us, then we of this world would surely experience a tremendous surge of mutual loyalty. Our international conflicts would die overnight as we marched (or rather "flew") together against such a common enemy. But not only is such an eventuality a pure phantasy, it also, from our present point of view, would be extremely undesirable. For such an interplanetary war would be far more horrible than our present mere earthbound wars. If we must have enemies against whom to unite, the only ones which can serve will be the intransigencies of inanimat  nature, on the one hand, and such minority human groups as seek to break away from the World State, on the other. And to fight these world enemies, we shall need a World Army or Police Force. This army or force must be a company of Sir Galahads ready to ride against all disruptive forces—whether they be recalcitrant rebel groups or the inanimate drags of ignorance, disease, fire and earthquake. This World Force must combine the hardness and the romance of the Northwestern Mounties with the ministering and scientific qualities of the International Red Cross. To adventurous and

idealistic youth all over the world such a Force would symbolize both the stern and the helpful fathers (or, perhaps, a gang of brothers). And in it, youth from all parts could be drafted, or enlist, without fear or favor. Such a Force would symbolize the authority and the loving care of parents and the democratic give and take of siblings.¹⁰

If the makers of the post-war peace have enough daring, enough unselfishness, and enough understanding of the "psychology of identification," then such a World State, to which we all could become overweaningly loyal, will be founded. If not, let us abandon hope.

¹⁰ Such a force would not only fight the *common enemies*, but also it would symbolize the early family set-up

It must be admitted that, anthropologically speaking, this is probably being far too glib about fathers, brothers, and siblings "all over the world" But essentially I believe what I am proposing to be basically correct.

THE CONCEPT OF SOCIAL STATUS

RAYMOND B. CATTELL

Harvard University

A. THE NEED FOR A BASIS FOR SOCIAL STATUS SCALES

Much writing and research, both in social psychology and in sociology, has been concerned in recent years with relating other variables to social status. For example, there have been long disputes as to the correlation of status with innate mental capacity, with delinquency proneness, and with reproduction rate. Some extremely important sociological processes, such as social mobility, depend entirely, conceptually, upon the definition of social status.

The time is certainly ripe, therefore, for an attempt to pass beyond the approximate and contingent notions, expressed by such confessions of confusion as "underprivileged" or "socio-economic" status, to a more precise concept. Until this is done, the manufacture of classificatory scales of social status should be classed with the dangerous occupations, dangerous in so far as concerted and realistic research is concerned. The present paper is an attempt to arrive at clearer orientation in the matter, from the standpoint of a psychologist.

B. THE CORRELATES OF SOCIAL STATUS

The view that social status may be determined from bank accounts, though attractively simple, has long been abandoned as too naive for sociological discussion. Since the dethronement of the economic index, other single indices have enjoyed a brief puppet authority. Thus, some researchers have measured status by cubic capacity of home per member of family, or by the number of servants kept, or by years of education. The reign of each has been more brief than its predecessors, until at last the cautious sociologist's concept of social status, seeking safety in numbers, has permitted itself to be governed by some aggregate of qualities, such as is implied by the term "socio-economic."

To take refuge in a cloud of particulars, however, is not to solve the nature of the general concept. Yet it does at least bring home to the specialists in *a priori* thinking who still haunt the social sciences that the general concept must be derived from the particular empirically discovered associations.

Before discussing techniques for abstracting the essential variant which lies behind the observed correlates of social status, it would be best to gain perspective by looking at the items which are alleged to vary with what has roughly been called social status. The following list contains some factors the correlation of which with social status has been proved, and defined within narrow limits, by extensive research. Others have only an association alleged from indirect reasoning. Some have so obvious a correlation that neither discussion nor research is needed to confirm their importance.

TABLE 1

CRITERIA OF SOCIAL STATUS AND THE CHIEF CORRELATED VARIABLES

1. Prestige of occupation, judged by attempts to reach it. (Positively related to higher status)
2. Size of income. (Positively related.) Also whether paid at longer (salary) or shorter (wage) intervals.
3. Possession of wealth or property and of unearned income. (Positively related.)
4. Length and type of education. (Positively related, over most of range.)
5. Possession of socially recognized title and privileges, *e.g.*, hereditary nobility ranks and professional titles.
6. Intrinsic characteristics of occupation: complexity, level of skill demanded, freedom from repugnant or banal qualities (as, *e.g.*, in scavenger's work, manual labor, etc.).
7. Mental capacity, as measured by intelligence tests. (Positively correlated, 7, 8, 15.)
8. Social status of those with whom the individual associates in private life, recreations, business.
9. Neighborhood, size and type of residence. (Positively related, 9.)
10. Amount of conspicuous expenditure on luxuries, recreation, arts, leisure (33), and matters beyond sheer living necessities generally. (Positively related over most of range, 9.)
11. Moral standards. (Greater crude delinquency at lower levels, 23. Less dogmatic standards at higher levels. Varying values with transition through all levels)

TABLE 1 (*continued*)

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12. Interests and aesthetic values as shown in reading (22, p. 381), recreation, hobbies, art forms.
 13. Code of manners, dress, food, and speech (accent).
 14. Relation to society in the occupational activities. Whether a public benefactor or in professional service, business (subdivision: buyer or seller), personal service, or in receipt of charity or public welfare assistance. In this category should be included the subtle persistence of traditional valuations arising from historical causes, *e.g.*, after long periods of warfare, when the warrior was *ipso facto* the commander and the possessor, the prestige of the military caste will persist out of relation to the indications from other social status correlates at the time.
 15. Political and religious affiliations. [Upper groups more inclined to believe rewards should be proportional to abilities, more individualistic, less communistic (28). In religion there is definite social grading of chief denominations (23), as judged by economic level, types of delinquency, and other criteria. From: Episcopal and Unitarian to Methodist, Baptist, and Catholic].
 16. Incidence of psychosis and nervous disorder. [Negatively related to status in most forms (34)].
 17. Incidence of physical disease and death-rate. (Negatively related to status in most respects.)
 18. Birth-rate. [Negatively related to status over whole range (6)].
 19. Marriage-rate. (Later mean age of marriage and lower marriage rate in upper classes.)
 20. Racial origin and appearance. (Related only in certain communities, *e.g.*, America, India, Italy. Stature and other physical features have a faint correlation in most communities.)
 21. Individual genealogy. This is somewhat similar to Nos. 5 and 6 above. Status through being related to some rich or eminent person or class of persons.
 22. Number of people to whom known. Unless the activities for which an individual or occupation is known are anti-social, the more celebrated tends to have higher status. Hartmann has shown that the unfamiliar occupation is underestimated, at least if it should really be above average (18).
 23. Age. There will be some appreciable correlation of age with social status, both because of occupational promotion and because of other social correlates of age and class (*e.g.*, greater longevity in upper classes).
 24. Sex ratio. (More women than men in upper class; more men than women in lower.) Cf. Warner's *Yankee City*.
-

It is possible to classify and subdivide these and lesser correlates in a variety of ways. Most interesting, from a functional point of view, would be a division into causes and consequences of social status difference. Much research would be needed to decide the importance of each connection when there is a circular relationship. Greater nervous instability, for instance (34), appears to be both a cause and a consequence of lower social status. The same may be said of birth-rate and of education.

Another division of particular interest is that into innate biological characters of classes on the one hand, such as average mental capacity and possibly some part of infertility, and acquired characters on the other, such as wealth and education. Or again, a division into characters largely transmissible from generation to generation, such as wealth, mental capacity, racial type, hereditary title, code of manners, and education, and those not normally transmitted, such as occupation, earned income, nervous disorder, political and religious affiliations, birth-rate, and physical diseases. Each purpose will suggest its own reclassification, but for most purposes the obviously useful selection is of more essential from less essential factors in social status. This problem of distillation will be met at the end of the following section.

C. THE FACTORIAL CORE OF SOCIAL STATUS

Now, a ready general agreement that these, and other factors yet unexplored, may correlate with social status in such a way as to permit a promising hierarchy or weighted composite scale to be designed for the purpose of assessment may lead us to overlook essential natural irregularities in our material. The fact that a good positive correlation exists does not prove that all the qualities concerned follow a normal distribution or that the relation between them is linear. We are not entitled to such easy assumptions as that social status is a continuum; that there are no gaps or belts of heavy concentration in various social latitudes, or that the change from unskilled workers to skilled workers is exactly in the same axis direction as the change from upper middle class to nobility. Obviously in some varia-

bles, such as titular privileges, there are definite discontinuities, while in others, such as income and size of family, the distribution curve is decidedly skewed.

A beginning in the matter of investigating possible discontinuities, particularly as they affect the association of individuals in circles of friends, clubs, and occupational activities has been made by the Lynds (22a), and in unpublished studies of the Yale Institute of Human Relations. There seems to be no conclusive proof, or even contributory evidence, that any definite gaps exist in the spectrum of social class, considered from the point of view of social intermixture and with respect to the social body of western democracies. But in relation to other variables and other societies, *e.g.*, that of India, it is obvious that discontinuities exist and that the term "social stratum" is accurate as a metaphor in its full geological sense.

We cannot find anywhere a perfectly homogeneous society in which social status is a quality grading regularly from one extreme to the other. Always there is intriguing irregularity: some difference in the width of strata, some discontinuity, some fracturing and faulty lie, and some vertical clearance planes adding complication to the horizontal division. The provocative studies of Murchison (24, 25) on *Gallus domesticus*, presenting one of the very few experimental approaches to the subject of social status, show that dominance hierarchies may exist in the barnyard, in a continuous form and with linear relations to other variables, but it would be a mistake to extend such conclusions unmodified to human societies.

Nevertheless, the fact that matter is not evenly distributed does not prevent our using the notion of the continuity of space. If we can find means to measure the dimensions of social latitude and longitude, the discontinuity and irregularity of the continents of social class in any given society should be no impediment to the use of precise descriptions of social status.

The axis along which social status is to be measured, however, must be decided by functional and other relations in the social body itself, for unless social status is something having meaning functionally in social life it is a useless concept. The

above discussion, indicating the existence of a considerable mass of correlated variables, suggests that the best way to get at this axis is by the method which has proved so valuable in individual psychology, namely, by factor analysis methods aimed at discovering whatever general factor or factors will account for the greater part of the variance in all these traits which loosely go together as "social status."

At certain levels of the social scale or in certain types of society the general factor isolated might be somewhat different. That is to say, any treatment of the problem which wishes to avoid brute mathematical mishandling of the organic realities of society must be prepared to find that the axis rotates a little in different societies or levels with corresponding alteration of meaning of the specific, though not of the general, concept and measurement of social status.

The internal consistency implied by the existence of a general factor is not the only reasonable criterion for shaping a concept of social status. It is obvious that for some purposes one of the correlated variables may be more important than another. For example, in economic problems the earning level of social classes affects calculations most, whereas in matters arising from biological endowment the level of free social intermixture, determining assortative mating, is the most important variable to enter the calculation.

However, it is not unfoundedly optimistic to expect that whatever general factor emerges from a factor analysis will also prove to have a functional reality and to enter into predictions, or to be a valuable reference concept in discussion, to a greater extent than any single variable.

To investigate this matter and carry out an adequate factor analysis would require a far more complete list of intercorrelations of social class attributes than is yet available. In the following preliminary attempt, however, we have taken what appear to be five of the most important definers of social status and intercorrelated them with respect to 25 occupations (listed later).

Occupations offer by far the most convenient points in the

social space continuum for use in establishing correlations, since many kinds of data are readily available with respect to them, but theoretically, at least, other items, *e.g.*, families, clubs, and isolated individuals, could be taken as points for the correlation matrix.

It is of interest from the point of view of method, however, to realize that the use of occupations, and indeed of any elements other than individuals, is vulnerable with respect to a persistent kind of psychological error, namely, that an assessor's attitude to people in a class is often very different from his attitude to them as individuals. Scientists are not unknown who speak highly of scientists as a class but are unable to point to any single scientist who meets with their approval. In several of the researches in the present bibliography the raters have been asked to rate the occupations for prestige, when those raters are too young to know much about the occupations and their relation to society and may not be actually acquainted with any individual in them. Rating of actual persons, according to social repute or prestige, sampled adequately from various occupations, and by raters well immersed in the social life of the adult world, would, therefore, seem to be the real foundation for research. Since the first part of our inquiry, however, is in any case inadequate to be considered as much more than a demonstration of method, being concerned with too few variables and cases to be a complete survey, we have assessed prestige directly from ratings of occupations as occupations.

The data for the correlations were obtained as follows: prestige or social repute standing from above student rating; average *IQ* of occupations from Fryer's study (15) and that of the present writer (17); years of education from a sampling of occupations by Super (30); size of family from data of the present writer (8). The last is scored in inverted form, *i.e.*, as birth control or restriction rather than as size of family or birth-rate, in order that all the correlations may be positive.

It is interesting to compare the correlations below with those yet available from other studies. Thus, Davidson and Anderson (13) found $r = 0.60 \pm .01$ for occupational status and years of

schooling; Counts (12) found $r = 0.73$ for intelligence and status; the writer (8) found $r = 0.91$ for familial intelligence and economic status, $r = -0.78$ for size of family and economic status, and $r = -0.84$ for (familial) intelligence and size of family. Naturally the precise size of these r 's is going to depend on whether occupations or individuals are taken as units and on the range of occupations used, but it is encouraging to notice that where these conditions are approximately equal (8) the r 's at least arrange themselves in the same order.

The examination of Table 2 of intercorrelations of Spear-

TABLE 2
INTERRELATIONS OF SOCIAL STATUS CRITERIA

	1	2	3	4	5	Saturation with general factor
1. Prestige rating		95	92	86	83	.98
2. Intelligence Quotient	95		89	86	91	.97
3. Income (annual)	92	89		82	81	.93
4. Years of education	86	86	82		82	.87
5. Birth restriction	83	91	81	82		.85

man's tetrad difference criterion (32) shows a good agreement of median of the tetrads with the calculated probable error, but not of r_{25} and r_{45} are included. Direct inspection is enough to show that these two r 's break the hierarchy. The five variables as made up of the following: (a) A single general factor with which we are dealing can, therefore, be considered (32) social status. (b) Two distinct group factors linking Birth Restriction with Intelligence and with Education (or possibly one group factor linking all three of these, since r_{24} is also unduly high). (c) Specific factors peculiar to each variable. The saturation of each of these variables with the general factor of social status is shown at the side of the above table. First, it is noticeable that the intelligence level (of those in an occupation) is almost as closely determinant of its social status as is its prestige in the eyes of the public. Secondly, the saturation of prestige with this general factor of social status is so high that for all practical purposes the nuclear element, prestige, can be taken as a measure of social status.

D. FURTHER STUDY OF THE CONCEPT: ITS INTERNAL VALIDITY

That the essence of socio-economic status should turn out to be a prestige factor will not surprise any who have given thought to the matter or observed social phenomena closely. It is this intangible index, rather than any index of income, property, or education, which determines the functional operation of social status, determining, for example, such matters as who shall mix with whom, what groups shall intermarry, what occupations shall be striven for, what individuals shall, *ex officio*, exercise greatest influence on society in general.

Social status, in short, is a purely psychological entity. Such a statement must not be taken to mean that it is not real or that it cannot be measured or that it is not a precisely definable concept. It is to be defined and measured in terms of behavior, implying mental states behind behavior. The prestige of an occupation is resident in the minds of all people in the community and is to be measured by assessing their attitudes towards it at a given time.

It might also be measured indirectly by observing the directions in which people try to move socially, plotting the stream lines in the flood of upward social mobility, constructing at right angles to them the lines of equal pressure, *i.e.*, of status on the map of occupations. Or it might be measured by some other consequence of prestige, such as potential influence in community affairs or power to produce suggestibility in others, as has been done by Bowden, Caldwell, and West (3). Or again, it might be measured, probably less reliably, by some indices of deference behavior, as between occupations, paired in every possible way.

A question which arises straightaway concerning the validity of considering status simply as prestige is this: Will the prestige ranking of occupations be agreed upon by observers at different levels of the social scale and with different degrees of acquaintance with the occupations concerned, or is prestige order always relative to point of reference? If the latter is true, then the various perspectives may or may not be resolvable into a

single construct taken to represent the "real" social stratification. To decide this, we had the above list of 26 occupations ranked first by 20 college graduates and then by 20 unskilled and skilled laborers. The ranking was carried out individually and with the help of cards, each bearing the name of an occupation, in order that the ranking might proceed by means of paired comparisons.

The agreement of these two orders was extremely good, the rankings being as indicated in Table 3.

TABLE 3
PRESTIGE RANKING OF OCCUPATIONS

Occupations	Graduate students	Laborers
Physicians	1	2
Banker, stock and loan broker	2	2
Superintendent of State institution	3	5
Captain in Army or Navy	4	4
Manager of business	5	1
Hotel keeper	6	7
Grade school teacher	7	10
Real estate and insurance agents	8	14
Retail trader	9	9
Commercial traveller	10	8
Bookkeepers, cashiers, and accountants	11	11
Foremen	12	6
Farm proprietors	13	16
Clerks and stenographers	14	12
Policemen	15	13
Skilled factory workers	16	15
Salespeople and clerks	17	17
Train, bus, and streetcar drivers	18	18
Waiters and domestic servants	19	20
Janitors	20	19
Laundry workers	21	24
Unskilled factory workers	22	23
Farm laborers	23	21
Casual laborers	24	25
Coal miners	25	22
Unemployed	26	26

The correlation by rank formula of these orders is $P=.94 \pm .16$.

The extent of the agreement between individuals as such may be gauged from the finding that the mean inter-individual correlation was 0.88 for the graduates and 0.63 for the laborers. The difference between these agreements is appreciable and suggests that education may be a factor in stabilizing ideas about social status.

The above agreement of students and laborers is about the same as that which Counts (11) found between various groups of school students, differing somewhat as to age and geographical location in the states. In his study agreement for seniors was 0.95; no correlation between group judgments fell below 0.90 and one reached 0.97. Neitz (26), repeating Counts' research after a decade, concluded that neither time nor distance (within one country) affect appreciably the ranking of occupations for status. Evidently perspective differences are slight and this method of achieving scales and measures of social status by measuring prestige attitudes in the general community is a valid one.

Some light may be thrown on the causes of perspective differences by glancing at the three occupations concerning which there is any remarkable difference of ranking. The group of higher social status ranks the real estate dealer, the cashier, and the accountant higher, while the laborers give unduly high rank to the manager of a business and especially to a works foreman. Evidently the workmen are giving excessive dimensions to the universe in which they happen to move, oblivious of the space beyond; while the group of higher status may be confusing its valuation of money with valuation of the person who handles it.

This suggests, incidentally, that the question as to whether familiarity raises or lowers status is not advantageously posed. Rather the question should be as to whether relative familiarity with certain patches of the social firmament increases the apparent dispersion of occupations in that region. Our preliminary indications suggest an affirmative answer.

Other causes of differences in perspective may be: (a) Varying valuations of the products or associated objects of an occupation from the standpoint of the needs of the occupation of the

rater, *e.g.*, higher rating of the banker by the entrepreneur or the gentleman of leisure, lower rating of the clergyman by the agnostic; higher rating of the specialist by the general practitioner (18). (b) Varying values arising from differences in temperament in raters. Bowden (3) found that women tend to rate educators more highly, while men esteem more the business man. Educators tend to rise in the hierarchy as the assessors become older (3). Some differences of occupational rating as between nation-racial groups, *e.g.*, the elevation of the militarist in some groups, of the priest in others, may have some relation to temperamental differences, but are more likely to come largely under the next category. (c) Differences in the cultural pattern to which the individual belongs will obviously alter the prestige ratings of occupations. Anthropologists have given innumerable examples of this. These pattern differences may be associated with natio-racial or with social sub-group patterns. When they are associated with social status itself it seems best to accept the ratings of status given by those living immediately in the range of status on which judgment is required.

E. USE OF THE CONCEPT IN SOCIAL STATUS SCALES

Obviously no single social status scale can be used in all communities, or in one community at widely different historical times, because of the wandering of occupations through the social range. But does the framework, within which the social status of an occupation or person can be fixed, remain constant for all communities? Again the answer is no. Study of the intercorrelations of social status criteria might reveal different saturations, or even different general and group factors, in different communities. For instance, economic level would not be so highly saturated with the general factor in Soviet Russia as it is in America; intelligence and status might not correlate so highly in a simple primitive community where physique is more important, such as that of the Australian Blacks, as in a complex civilization; birth restriction would correlate less with status in medieval than in modern England, and so on.

However, on general psychological grounds it is possible to make a convincing case for the statement that whatever clusters and group factors would constitute the core of social status, the variable "prestige" is likely to be very central and to offer the best single measure. The statement, of course, needs to be justified empirically, by making factor analyses for many communities and cultures and observing which criterion has the highest and least variable saturation. The *a priori* grounds for supposing that prestige is likely to constitute the most invariable standard are, in a word, that in a sex inhibited and materially secure community, such as most Christian, industrialized nations, the drive towards prestige and attainment is more powerful and widespread than any other. The tendency is for other purposes to group themselves around it, as means to that end, in so far as the community pattern approves of their being considered means to that end, *e.g.*, some may not approve of wealth as being considered a means, some hereditary titles, some the number of wives, and so on.

Historically, for example, it is possible to detect other motifs in social status. Originally prestige appears attached to individuals on account of personal qualities (20) and the ability to distribute largesse. It is also obviously tied up with such discrete classes as are connected with age and sex divisions, legitimacy and illegitimacy, freedom and slavery. But with the increasing organization and complexity of society these badges of prestige arrange themselves hierarchically around the central theme of pure status.

From the consideration of facts and arguments in this article it would seem desirable to abandon as foundations for a social status scale such bases as economic level or complexity of occupations. The scales of Taussig (31), Nystrom (27), Sims (29), are largely oriented with regard to economic (budget or earnings) and standards of living. That of Counts (12), with this more or less derived from it (2, 4), use a composite of economic level, social status and intellectual outlook, but lean a little too much on similarity of occupation as judged from a census viewpoint. With these should be classed the Edward's

Scale (14), which is highly convenient, since its subdivisions correspond well with census divisions, but which is more logical than real and psychological in its rankings. [For example, "Professional persons" of all grades come above all kinds of "Proprietors"; "Farmers (owners and tenants)" rank above "Clerks and kindred workers."]

The work of Burt (5), of Barr (1), and of Brussell (4) approaches most closely to the concept developed here, for these researchers took as their criterion the intelligence level of the occupation (measured or assessed), which, as we have seen, is practically as highly saturated with the general factor of social status as is prestige itself.

It is the opinion of the writer that a slight modification of these scales, altering the few items which show a discrepancy of prestige and intelligence level, would provide, pending extensive correlation research, a reasonably exact scale of true social status. Such a scale, with modifications based on the present research

TABLE 4
PROPOSED METRIC SCALE OF SOCIAL STATUS

Broader grouping	Essential grouping	Occupations typical of the group
Upper class	Social Grade I	Positions of eminence in government, army, navy, church Aristocratic title Outstanding positions in the higher professions (physicians, lawyers, professors) Directorships of large industrial concerns
	Social Grade II	Professional occupations (surgeons, lawyers, teachers, engineer surveyors, clergymen) Managers of larger businesses and of factories Chief officials in local government and in charge of institutions Bankers and stockbrokers Captains and similar ranks in military forces

TABLE 4 (*continued*)

Broader grouping	Essential grouping	Occupations typical of the group
Upper middle class	Social Grade III	Grade school teachers Accountants, lesser professional occupations, <i>e.g.</i> , veterinary surgeons, dentists Ship's captains Proprietors of business, <i>e.g.</i> , hotel keepers
	Social Grade IV	Insurance agents Real estate agents Wholesale traders and merchants Airplane pilots
Middle class	Social Grade V	Foremen among workmen Commercial travelers Cashiers in commercial houses
	Social Grade VI	Retail traders (proprietors) Stenographers Clerks Farmer proprietors
Lower middle class	Social Grade VII	Skilled factory workers Skilled workers Policemen Nurses
	Social Grade VIII	Farmer tenants Sales clerks Bus and train drivers Semi-skilled workers
Lower class	Social Grade IX	Unskilled workers, <i>e.g.</i> , laundry workers Farm laborers Coal Miners Porters or waiters Janitors Trained domestic servants
	Social Grade X	Casual laborers Unemployed Institution inmates

data and intended for use in Atlantic democratic communities, is suggested below. It is divided basically into 10 divisions but there are no rubrics for these divisions other than in terms of class level; for occupational or economic divisions as such, *e.g.*, professional, proprietorial, are, as argued above, only falsely applied to social status and extend over too wide a range of status to be meaningful.

If the terms of the broad grouping into five divisions are employed it is necessary that they should not be confused with the expressions "middle class," "upper class", etc., in popular parlance. A habit of euphemism and a collection of misconceptions as to how "the other half" of the social world lives has obviously deprived the popular phrases of any scientific meaning. The popular "middle class" is certainly far above the middle, indeed upper and middle together probably constitute less than a third of the population. In general there seems to be no advantage sufficient to outweigh the disadvantages of using terms in present use, however racily empirical, by which class attempts to describe class, *e.g.*, "Highstreeter and sidestreeter," "Bourgeois," "Nouveau riche," "Upper ten," and "Submerged tenth," etc.

The present scale could have the magnitude of its divisions defined either as subtending equal intervals along the dimension of social status, or as cutting off equal fractions of the population, *i.e.*, constituting centiles and deciles. The former seems more desirable, for if our concern is with the dimension of social status we are naturally interested in utilizing it to the fullest extent by working in equal units. Since it seems reasonable to assume, contingently, that occupations, like other small groups, or like individuals, are distributed according to a normal Gaussian curve with respect to social status, the five social class divisions would cut off the following percentages of the population of persons or occupations: upper 7, upper middle 25, middle 36, lower middle 25, lower 7. The ratios of these parts, noticeably different from those of the classes as popularly labelled, could be empirically checked by taking equal intervals on a rated, complete series of occupations, spaced in rating by

the same technique as used by Thurstone for attitude test items, or measured more accurately on the general factor in a complete set of criteria of social status.

REFERENCES

- 1 BARR, F. E. A scale of measuring mental ability in vocations and some of its implications. In *Genetic Studies of Genius*, L. M. Terman (ed.) Vol. I. Stanford University, Calif. Stanford Univ. Press, 1925
- 2 BECHMAN, R. O. A new scale for gauging occupational rank. *Person. J.*, 1934, 13, 225-233
- 3 BOWDEN, A. O., CALDWELL, F. F., & WEST, G. A. A study in prestige. *Amer. J. Sociol.*, 1934, 41, 193-204
- 4 BRUSSELL, E. S. A revision of the Barr-Taussig scale. Univ. Minn. (Unpublished.)
- 5 BURT, C. A study in vocational guidance. London *Indust. Res. Board.* 1926, No. 33.
- 6 CATTELL, R. B. Is national intelligence declining? *Eugen. Rev.*, 1936, 28, 181-203.
- 7 ———. Occupational levels of intelligence, and the standardization of an adult intelligence test. *Brit. J. Psychol.*, 1934, 25, 1-28.
- 8 ———. Some further relations between intelligence, fertility, and socio-economic factors. *Eugen. Rev.*, 1937, 29, 171-179
- 9 CHAPIN, F. S. A quantitative scale for the rating of homes, and social environment of middle class families; a first approximation to the measurement of socio-economic status. *J. Educ. Psychol.*, 1928, 19, 99-111.
- 10 ———. Socio-economic status: some preliminary results of measurement. *Amer. J. Sociol.*, 1931, 37, 581-587.
- 11 COUNTS, G. S. The selective character of American secondary education. *Univ. Chicago, Supplementary Educ. Monog.*, 1922, No. 19.
- 12 ———. The social status of occupation; a problem in vocational guidance. *Sch. Rev.*, 1925, 33, 16-27.
- 13 DAVIDSON, P. E., & ANDERSON, H. D. Occupational Mobility in an American Community. Stanford University, Calif.: Stanford Univ. Press, 1937.
- 14 EDWARDS, A. M. A social-economic grouping of the gainful workers in the United States. *J. Amer. Statis. Assoc.*, 1933, 28, 377-387.
- 15 FRYER, D. Occupational intelligence standards. *Sch. & Soc.*, 1922, 16, 276.
- 16 GOODENOUGH, F. L., & ANDERSON, J. E. *Experimental Child Study*. New York: Century, 1931.
- 17 HARTMANN, G. W. The prestige of occupation. *Person. J.*, 1934, 13, 144-152.
- 18 ———. The relative social prestige of representative medical specialties. *J. Appl. Psychol.*, 1936, 20, 659-663.
- 19 KEFAUVER, G. N., NOLL, V. H., & DRAKE, C. E. The secondary school population. *National Sur. Sec. Educ.*, 1933, No. 17.
- 20 LANDTMAN, G. *The Origin of the Inequality of the Social Classes*. New York: Prentice-Hall, 1938.
- 21 LORIMER, F., & OSBORN, F. *The dynamics of population*. New York: Macmillan, 1934.
- 22 LUNDBERG, G. A., & STEEL, M. Social attraction patterns in a rural village: A preliminary report. *Sociometry*, 1938, 1, 375-419.

- 22a. LYND, R. S., & LYND, H. M. Middletown New York: Harcourt & Brace, 1929.
23. MURCHISON, C. Criminal Intelligence Worcester, Mass.: Clark Univ. Press, 1926.
24. ——— The experimental measurement of a social hierarchy in *Gallus domesticus*: III. The direct and inferential measurement of Social Reflex No. 3. *J. Genet. Psychol.*, 1935, 46, 76-102.
25. ——— The experimental measurement of a social hierarchy in *Gallus domesticus*: VI Preliminary identification of social law. *J. Gen. Psychol.*, 1935, 13, 227-248.
26. NEITZ, J. A. The depression and the social status of occupation *Elem. Sch. J.*, 1935, 35, 454-461.
27. NYSTROM, P. H. Economic Principles of Consumption. New York: Ronald Press, 1929.
28. PEARL, R. A classification and code of occupations. *Human Biol.*, 1933, 5, 491-506.
29. SIMS, V. M. The Measurement of Socio-Economic Status. Bloomington, Ill.: Public School Publishing, 1928.
30. SUPER, D. E. Occupational level and job satisfaction. *J. Appl. Psychol.*, 1939, 23, 547-564.
31. TAUSSIG, F. W. Principles of Economics. (3rd ed.) New York: Macmillan, 1928.
32. THOMPSON, G. H. The Factorial Analysis of Human Ability. New York: Houghton Mifflin, 1939.
33. VEBLEN, T. Theory of the Leisure Class New York: Macmillan, 1919.
34. ZUBIN, J. The economic aspects of mental disease. *Amer. Assoc. Advancmt. Sci.*, No. 9.

IDENTIFICATION WITH SOCIAL AND ECONOMIC CLASS

HADLEY CANTRIL

Princeton University

In a recent series of addresses, Pear points out that the problem of social stratification has remained almost unexplored by social psychologists.¹ It may well be that one basic reason for this neglect has been the traditional class character of the social sciences themselves.²

Of late the chief contributors to our knowledge of class structure have been the social anthropologists, especially Lloyd Warner and his group in their studies of Yankee City.³ Dollard has done pioneer work in the field using more psychoanalytic concepts.⁴ Nevertheless, Pear's stricture is still valid. Social psychology needs more observation, more data, more systematic thinking on the problem of social stratification. It is in this context that the ideas here are presented.

At the present time and in our present culture, one psychological problem of class structure concerns the individual's identification of himself with a certain economic group and with a certain social class. From everyday experience we know that

¹ T. H. Pear. Psychological aspects of English social stratification. *Bull. John Rylands Library*, 1942, 26, No. 2. Pp. 27.

² For a statement of this point of view and a stimulating discussion of class structure by a Marxian sociologist, see Nikolai Bukharin, *Historical materialism: a system of sociology*. New York: International Publishers, 1925.

³ W. Lloyd Warner and Paul S. Lunt. *The social life of a modern community*. New Haven: Yale Univ. Press, 1941.

⁴ John Dollard. *Caste and class in a Southern town*. New Haven: Yale Univ. Press, 1937.

such identifications are made and we also suspect that there is by no means always total overlapping between the two. Evidence concerning such identifications and possible discrepancies should at least fill a small gap in our knowledge of class structure. Furthermore, if comparable evidence could only be gathered at different time intervals in the progress of a given culture and in different cultures, a beginning would be made of a broader, more inclusive study of class structure, analyzing changes through the decades and the differential effect of events on the many existing forms of society. It would, for example, be of considerable value to social scientists of various disciplines to know how the war was changing the class structure of England and the United States, not to mention Germany, Italy, China, or even the Soviet Union.

METHOD

The Office of Public Opinion Research at Princeton has asked the following two questions of representative samples of the national population: ⁵

Which income group in our country do you feel that you are a member of — the middle income group, the upper income group, or the lower income group?

...Upper ...Upper middle ...Middle ...Lower middle ...Lower
To what social class in this country do you feel you belong — middle class, or upper, or lower?

...Upper ...Upper middle ...Middle ...Lower middle ...Lower

The figures reported below are taken from a survey made in June, 1941.⁶ Altogether 3114 complete interviews were made. The same ballot contained the usual interviewers' ratings on economic status, and in addition this particular ballot asked people "Is your family income under \$15 a week, between \$15 and \$20 a week, between \$20 and \$40 a week, between \$40 and \$60 a week, or over \$60 a week?"

⁵ The Office, of which the writer is Director, uses the interviewing facilities of the American Institute of Public Opinion (Gallup Poll).

⁶ Two other surveys have included these same questions. So far no significant trends have appeared. When more cases have been accumulated it will be possible to study the various groupings in more detail and discover something of their composition by occupation, age, party affiliation, and other controls regularly used.

The two questions on class identification were deliberately framed in general terms. No further definition was provided. Interviewers were instructed not to help in the interpretation of the questions if help was solicited. That the questions were meaningful to respondents is partially indicated by the small number unable to answer them; 3 per cent could not tell what social class they belonged to, only 1 per cent could not fit themselves into an income group. Interviewers also reported little difficulty in getting answers to the questions.

RESULTS

The results of the survey are shown in Tables 1-7 below. The most significant findings revealed in the tables are indicated under the *conclusions* appearing directly after the tables.

TABLE 1 *
PERCENTAGES OF SAMPLE POPULATIONS WITH SOCIAL CLASS AND
INCOME GROUP IDENTIFICATIONS

INCOME GROUP IDENTIFICATION		SOCIAL CLASS IDENTIFICATION	
1.4	Upper	4.9	
6.9	Upper Middle	10.5	
41.3	Middle	65.8	
23.5	Lower Middle	11.1	
26.9	Lower	7.7	
100.0		100.0	

* Although decimals in figures such as these with probable errors of at least 3 or 5 per cent are meaningless, they are included here since in some of the categories less than 1 per cent of the cases are found.

TABLE 2
RELATIONSHIP OF INCOME AND SOCIAL IDENTIFICATIONS
(Percentages of Each Social Class in Various Income Groups)

SOCIAL CLASS IDENTIFICATION	INCOME GROUP IDENTIFICATION					
	U	UM	M	LM	L	
Upper	17	10	37	13	23	100
Upper Middle	3	45	32	15	5	100
Middle	0.3	2.4	54	22	21.3	100
Lower Middle	0	0.3	5	58	36.7	100
Lower	0	0	4	4	92	100

TABLE 3

RELATIONSHIP OF INCOME AND SOCIAL IDENTIFICATIONS
(Percentages of Each Income Group in Various Social Classes)

SOCIAL CLASS IDENTIFICATION	INCOME GROUP IDENTIFICATION				
	U	UM	M	LM	L
Upper	63	7.5	4	3	4
Upper Middle	22	69	8	7	2
Middle	15	23	86	62	53
Lower Middle	0	0.5	1	27	15
Lower	0	0	1	1	26
	100	100	100	100	100

TABLE 4

DEGREE AND DIRECTION OF DISCREPANCY BETWEEN SOCIAL AND
INCOME IDENTIFICATIONS

DISCREPANCIES	PERCENTAGE OF TOTAL POPULATION
Same income and social class	54.3
SOCIAL class regarded as one step higher	22.6
SOCIAL class regarded as two steps higher	17.6
SOCIAL class regarded as three or more higher	2.3
INCOME group regarded as one higher	2.7
INCOME group regarded as two or more higher	0.5
SOCIAL class regarded as ONE OR MORE STEPS HIGHER	42.5

TABLE 5

WEEKLY INCOME BY SOCIAL CLASS IDENTIFICATION

SOCIAL CLASS IDENTIFICATION	ACTUAL FAMILY INCOME				
	\$60+	\$40-\$60	\$20-\$40	\$15-\$20	—\$15
	Per cent	Per cent	Per cent	Per cent	Per cent
Upper	28.2	18.3	27.5	9.9	16.1
Upper Middle	34.9	30.0	23.0	8.2	3.9
Middle	9.3	18.1	36.8	18.4	17.4
Lower Middle	1.9	4.7	27.1	27.4	38.9
Lower	.5	3.2	24.1	22.2	50.0

TABLE 6
WEEKLY INCOME BY INCOME GROUP IDENTIFICATION

INCOME GROUP IDENTIFICATION	ACTUAL FAMILY INCOME				
	\$60+	\$40-\$60	\$20-\$40	\$15-\$20	—\$15
	Per cent	Per cent	Per cent	Per cent	Per cent
Upper	85.8	9.5	2.4	2.3
Upper Middle	47.1	33.0	16.6	2.9	.4
Middle	14.5	26.1	38.2	13.6	7.6
Lower Middle	3.1	11.7	41.1	26.1	18.0
Lower	1.5	3.5	23.1	22.8	49.1

TABLE 7
SOME CORRELATIONS BETWEEN IDENTIFICATIONS AND
ECONOMIC INDICES

REACTIONSHIP	PRODUCT-MOMENT <i>r</i>
1. Social class and income group identifications	.49
2. Income group identification and interviewers' ratings on economic status	.60
3. Income group identification and weekly income	.58
4. Social class identification and interviewers' ratings on economic status	.41
5. Social class identification and weekly income	.37
6. Interviewers' ratings on economic status and weekly income	.73

CONCLUSIONS

1. *The overwhelming majority of the American people identify themselves with some category of the great middle class.*

a. Almost nine-tenths feel themselves members of some form (upper middle, middle middle, or lower middle) of the middle social class (Table 1).

b. Almost three-fourths believe themselves members of some form of the middle income class (Table 1).

2. *There is by no means a close one-to-one correspondence between social class identification and income group identification.*

- a. The product-moment correlation is only .49 (Table 7).
- b. There is strict correspondence between the two identifications with only 54 per cent of the sample population (Table 4).
- c. Higher correlations were obtained between weekly income or interviewers' ratings of income with *income* group identification than between weekly income or rating with *social* group identification (Table 7).

3. *The pull toward identification with the middle social class is greater among lower-income people than among upper-income people.*

- a. Only 37 per cent of the group believing they are *upper* income place themselves in any middle-social-class category (Table 3).
- b. Seventy per cent of those who call themselves members of the *low* income group identify themselves with some form of middle social class (Table 3).

4. *There is a distinct tendency for people to regard their social class as higher than their economic group.*

- a. Over 40 per cent of the population fit themselves into a higher social than income group (Table 4).
- b. Whereas 50 per cent think of themselves as below the middle-middle *income* group, only 19 per cent think of themselves as below the middle-middle *social* group (Table 1).
- c. Nearly one-fifth of the population believe their social class is at least two steps higher than their income group (Table 4).
- d. Only 3 per cent feel their income group is higher than their social class (Table 4).

5. *The disparity between social class and income group identification increases as one goes up in social class or down in income group.*

- a. The higher a person's social class identification, the more is he likely to feel that his income group is lower than his social class (Table 2).
- b. The lower a person feels his income group to be, the

more is he likely to believe that his social class is higher than his economic class (Table 3).

c. Psychological identification with the upper social class seems relatively independent of income. The upper social class is more evenly distributed by income groups. In other words, an admission that one has no money does not necessarily make one feel a member of a low social class (Table 2).

d. On the other hand, those in the lower social scale are almost all in the lower-income group according to their own evaluations. In other words, an admission of low social class membership is also an admission that one is poor (Table 2).

6. There are several clear social implications shown in these data on class structure in present-day American society.

a. There is a widespread discrepancy between the level of social aspiration and the income necessary to solidify one's position.

b. Since greatest disparity between income and social identification exists among low-income groups and since there is a high correlation between identification in an income group and actual income, there is undoubtedly more tension and frustration among the low-income group elsewhere.

c. Although a person's economic status is positively related to his social class identification, there is enormous fluctuation between the two. Many other criteria than income are used by individuals in our culture in their class identifications.

d. As implicitly defined by the public, the higher the social class, the more is it likely to be founded on non-economic criteria. The higher the social class, the greater appears to be its complexity in terms of the number of possible non-economic criteria available for identification. Persons in the low social class are there in large measure because they are poor with all that poverty implies, while people who feel they are in the upper social class are not necessarily there because they feel they are well off—many other qualifications such as family connection, occupation, accomplishment, education, and the like make upper social identification possible. For example, 27% of those who place themselves in the upper social class earn less than \$60 a week (Table 5).

AN EXPERIMENTAL APPROACH TO THE STUDY OF MOB BEHAVIOR

NORMAN C. MEIER, G. H. MENNENGA,¹ AND H. J. STOLTZ²

University of Iowa

While experimental attack on the more violent types of crowd behavior, as exemplified in the lynching mob, presents almost insuperable difficulties, modifications of experimental procedure may be contrived which are likely to yield findings of considerable scientific value. The obvious difficulties in any study of crowd behavior are chiefly those of recording a reliable response under the stress of fast-moving events, and of being able to anticipate an actual mob event. The use of motion pictures offers an objective and descriptive technique, but provides little information regarding the motivation of specific individuals.³ The technique is also deficient in that it cannot offer much if any real data on the antecedent motivation of the individual—which, in the writers' belief, is fundamentally necessary for any understanding of a crowd episode.

It was believed feasible, however, to simulate a crowd situation with such fidelity to detail as to produce in the subjects a genuine emotional reaction of such depth as to permit immediate investigation of response-dispositions and motives in each subject before he should begin to suspect the true character of the event. If such an attempt could be carried out successfully, the pro-

¹ Western Theological Seminary, Holland, Michigan.

² Community High School, Normal, Illinois.

³ Those in news shorts are too abbreviated, while those acted out tend to be artificial.

cedure should disclose valuable data on probable individual participation in mob activity and on the usual motives functioning in each particular type of reaction.

Based somewhat on a plan devised by one of the authors earlier (8), a scheme was first devised to anticipate all the normally possible reactions likely to be disclosed and also, from a study of voluminous accounts of mob behavior in various parts of the United States, to reconstruct a motivation-schedule to fit each of the separate categories of response. The approach then would be to synchronize the events of the crime with local persons, places, and establishments in such manner that all the events would have reached their culmination at the time the subjects were assembled. The actual incitation would then take the form of the "leader's" appearing unexpectedly, greatly excited, with information about the "crime" in the form of either a sheaf of news bulletins which he had just secured at the news office (first experiment), or a newspaper extra (second experiment) which he would immediately read to the assembled subjects, having previously prepared himself to deliver the information in the most dramatic (astounded, deeply moved) fashion. Not only would every device be employed to invest the simulated crime with appearance of reality, but the "criminals" would supposedly be brought within easy reach of the assembled subjects. At this point, there would be introduced the recording device heretofore concealed and sealed against inspection, with a plausible reason assigned for its introduction; and the response then would be drained off within a brief moment, after which the subjects would be disillusioned.

THEORETICAL CONSIDERATIONS

The studies reported herein attempt to test the hypothesis advanced by F. H. Allport (1) and others that in the crowd the individual does not, as the French imitation school maintains (6), become a creature of irrational hate, capable of any excesses, including the taking of human life; but rather acts in accordance

with the dictates of past habit and attitude formation, only, as Allport suggests, possibly to greater degree. Applying this hypothesis to the lynching situation, one would say that an individual reared in the deep South, even though a church-goer, finds the ascendancy of those social norms concerned with white supremacy and the inviolability of white womanhood such as to predispose him toward participation in a Negro lynching; whereas the same individual, reared in a Minnesota community and not having these social norms, would be more inclined to favor the normal process of law. The thesis would also maintain that, in the latter instance, the behavior of the individual would be, to some degree, affected by the degree of certainty of evidence against the suspected victim. If the thesis is sound, it would also follow that efforts to deter mob action would not only be in accordance with present attitudes, but would vary in some degree with the degree of guilt or the validity of evidence against the suspect. In no instance would the thesis admit that all individuals, regardless, would act according to a single pattern, moving precipitately into drastic action on finding themselves under the "contagion" of mob emotionality.

FIRST EXPERIMENT ⁴

A preliminary study of individuals participating in mob action was made by scrutinizing press accounts and the literature descriptive of recent mob action in various parts of the country (2; 3; 6; among others). This material was supplemented by contacts with mob participants in a lynching which had just occurred near Maryville, Missouri. In an attempt to ascertain motivating incentives and past attitudes toward Negroes, as well as other pertinent considerations in the motivating complex of each individual, Mr. Mennenga interviewed scores of persons in all walks of life who had been present, including actual participants. He also interviewed influential individuals in the community who were not spectators or participants, in order to de-

⁴ While planned in Iowa City, the actual events of this experiment were carried out in Pella, Iowa, with the cooperation of the faculty and students of Central College, arranged by Mr. Mennenga.

rive some estimate of the social milieu of the particular locality. There was also obtained by the director of the study a documentary account, through cooperation of the Department of Sociology of the University of Missouri, of a lynching on the outskirts of Columbia, Missouri, shortly after the event.

From these data it was decided that it would be possible to construct an imaginary kidnaping episode, beginning in nearby Des Moines and terminating in Pella, Iowa, which, in its final form, should be reduced to a series of news dispatches reported to have been received at that time in a local office of a Des Moines newspaper, together with information just obtained from the Police Department.

In order to vary the conditions of the experiment, it was decided to use three groups, all normally meeting the same evening in successive order, and to vary the details of the kidnaping in such manner as to produce: (1) the detailed account of the kidnaping, the flight of the kidnapers toward Pella, their apprehension in Pella, with the terminating condition that the evidence was entirely circumstantial; (2) the events as in (1), with the additional increments of strong evidence, but still chiefly circumstantial; and (3) the events as in (1) and (2), but with positive evidence, including confession, of the identity and guilt of the culprits.

The motivation-analysis form was prepared in mimeograph in six pages, only three of which were to be used by any subject. Page 1 simply listed four courses of action for the individual to select one, as follows:

- () 1. I would join the crowd immediately.
(Now turn immediately to page 2)
- () 2. I would be unwilling to be actually involved but would go along to assist the others.
(Now turn immediately to page 3)
- () 3. I would not participate in any way.
(Now turn immediately to page 4)
- () 4. I would attempt to reason with the mob to avoid hasty action.
(Now turn immediately to page 5)

After the person had checked the course of action which he was at that moment about to take, he then turned to the appropriate

section indicated, where were listed all of the normally motivating impulses which would urge him to do what he was about to do. For example, in connection with the choice of action number one, several of the motivating incentives were as follows:

- Yes No 1. I would help, thinking that the law might not convict them.
- Yes No 2. I would help so as to make this case an example for all potential criminals of this sort.
- Yes No 3. I would help because if this crime should go unpunished innocent children would be unsafe everywhere.

In the event the person selected course number three (no participation), he might check such reasons as these:

- Yes No 1. I would not participate because it would mean a "blot on the community" to have someone lynched.
- Yes No 2. I would not participate because violence nauseates me.
- Yes No 3. I would refuse to participate since there is always the possibility of becoming involved in legal proceedings as an accessory or a witness.

When he had checked his reasons, the person then responded to questions in a final section, which were designed to register the emotional reactions of the individual as near as possible to the state he was in when the procedure was interrupted; also information of earlier contacts, if any, with mobs, and considerable information on such matters as religious affiliations, nationality, home state, and considerations not listed, but present.

The three groups of approximately 40 each met at 7 P.M., 8 P.M., and at 9 P.M. on the same evening. All subjects were in total ignorance of the experiment. The recording forms, sealed with a covering blank sheet, had been distributed around the room earlier, with a simple instruction that they were not to be used until instruction was given. Since these groups normally participated in exercises of various kinds, there was no reason to believe that this procedure created any untoward curiosity.

Theoretically the procedure had the following potentialities of the crowd situation:

- (1) The presence of a leader who, by dynamic manner and emotionally charged language, would secure and hold the attention of the group.

- (2) A focalization of attention comparable to the unanimity of crowd rapport.
- (3) Exceedingly high interest value in the subject matter, identical with actual possible content.
- (4) The possibility for various accompanying crowd phenomena, such as circularity, social projection, etc.

In actual practice it lacked several important aspects:

- (1) The subjects were all of average or above average intelligence.
- (2) There was lacking the emotionally unstable, the illiterate, and the individual highly impregnated with attitudes making him readily amenable to mob violence.
- (3) The experiment is dependent upon persistence of genuine crowd emotionality for adequate recording to take place, in order to yield a trustworthy register of the actual emotionality.⁵
- (4) There was presumably some variation in the degree of sincerity and candor in the response.⁶

In the Illinois study, the options in electing the course of action were increased to five, as follows:

- (1) Join the mob immediately to take an active part.
- (2) Join the crowd immediately to take a minor part.
- (3) Join the crowd immediately to see what happened.
- (4) Join the mob immediately to reason with the crowd to avoid hasty action.
- (5) Not to join the crowd.

The Illinois study differed also in that a faked newspaper extra was introduced to make the simulation more plausible and convincing. The "extra" was prepared in the office of a local newspaper, with large headlines, three decks of subheads, and a two-column, vivid account of the episode. For purposes of indicating the graphic character of the stimulus material, the headlines, decks, and first two paragraphs are reproduced below: The entire article was prepared with every detail synchronized with actual scenes, settings, and people in the community, so that even the lurid account of the mob's activities up to the point of the "extra" had every appearance of genuineness. In the Illinois study, an effort was also made to discover from tests given

⁵This aspect was checked carefully in the second experiment.

⁶This was provided for in some measure by anonymity and assurances as to the confidential nature of the record.

BLOOMINGTON SCHOOL GIRL KIDNAPED

**GANG MEMBERS CAPTURED
AFTER THRILLING CHASE**

FIND VICTIM IN LUGGAGE TRUNK

**Sheriff, Police Battle
Enraged Mob All
Night Long**

After a night of horror, during which a mob of 3,000 persons stormed the McLean county jail in an effort to seize the confessed kidnapers of Mary Jane Froman, local school girl, the City of Bloomington is still in a state of turmoil this morning. The kidnapers, giving their names as Anton Caznoeri and Al Druggan, both of Peoria, were captured at 8:30 last night by Sheriff Reeder and his aids after a thrilling ten-mile chase. The bruised and broken body of the Froman child was later found in the luggage compartment behind the rear seat in the kidnapers' automobile in what appears to be one of the most

brutal crimes ever committed in central Illinois.

Seized on Way from School

Seized by the kidnapers on her way home from school between 3:30 and 4:30 P. M., yesterday, Mary Jane, ten-year-old daughter of George Froman, wealthy Bloomington manufacturer, was held for \$20,000 ransom. Answering the telephone in his West Front Street office at 5.00 P. M., Froman was informed that his child had been kidnaped and warned, it is reported, that any attempt to communicate with police or newspapers would result in her immediate death. He was also told that a note would be found on the steps of the side entrance of the company's warehouse. Efforts to trace the mysterious call proved fruitless.

Under a mat

before and subsequent to the event of the intelligence level, emotional stability, and other personality traits of the subjects, particularly those who would take some active part in the mob itself. Certain modifications were introduced in the Illinois material to produce the effect of absolute certainty of guilt, as contrasted with the fair certainty version.

Subjects. In the Pella (Iowa) study, 124 subjects were used, 79 men and 45 women, all college students, with a mean age of 20.39 and an age range from 17 to 28. In the Illinois study, 121 subjects were used, 46 men and 75 women. Four subjects, however, failed to make the differential response indicating the course of action selected, which leaves 117 complete records. The subjects ranged from 17 to 44 years, with the mean age of 20.9.

RESULTS: PELLA STUDY

The two experiments yielded three types of data: (a) quantitative breakdowns of the response about to be undertaken under variations of the incitation materials, (b) indication of the functioning of incentives, and (c) retrospective evaluations and information.

TABLE 1
SUBJECTS' ABORTED RESPONSE UNDER CONDITIONS OF INDICATION

		CHARACTER OF EVIDENCE		
RESPONSE (OPTIONS)	N	UNCERTAINTY	FAIR CERT.	ABSOLUTE CERT
1 . . . join immediately	19	2	3	14
2 . . . go along	19	8	4	7
3 . . . stay away	41	14	16	11
4 . . . prevent	45	20	16	9
Totals	124	44	39	41

It is evident that freshmen as a class and men tend toward participation out of proportion to their numbers, compared to other classes and to women, respectively. The numbers are too small, however, to warrant further statistical treatment.

Primary Motivations

For Option 1 (Join immediately). In the case of the two subjects "set" to join in spite of the fact that only meager evidence has been established against the culprits, there was evident all through their detailed responses a very decided attitude to let nothing prevent their doing violence to the abductors; almost

TABLE 1-A

ANALYSIS OF REACTIONS ACCORDING TO ADVANCEMENT IN COLLEGE TRAINING

		OPTION															
		I (JOIN)				II (OBSERVE)				III (AVOID)				IV (DETER)			
CLASS	N	A ¹	B ²	C ³	T ⁴	A	B	C	T	A	B	C	T	A	B	C	T
Fr.	46	2	2	6	10	2	2	3	7	5	5	4	14	6	6	3	15
So.	24	0	1	2	3	3	1	2	6	2	0	1	6	4	4	1	9
Jr.	31	0	0	4	4	1	0	1	2	5	3	4	12	6	4	3	13
Sr.	23	0	0	2	2	2	1	1	4	2	5	2	9	4	2	2	8

¹ Uncertainty regarding guilt.² Fair certainty.³ Absolute certainty.⁴ Total.

TABLE 1-B

REACTIONS BY SEX

	N	OPTION			
		1	2	3	4
Men	79	14	11	18	36
Women	45	5	8	23	9

every conceivable reason was checked by both men. The same is true of the three who would join when fair certainty of the guilt had been established. They wished to help make other children safe, fearing that if the abductors were not removed, they might again be at large in society.

Of the 14 ready to join the group immediately upon absolute evidence, the two primary motives are, first, that, should these abductors go unpunished, children everywhere would be unsafe; and, secondly, that the miscreants might even kidnap another person, possibly a near relative.

For Option 2 (Reluctant to participate but would go along). These individuals would hesitate because of the following considerations: fear of not having the right persons; the belief that it is wrong for a crowd to take the law into its own hands; a distaste for taking human life based upon their conception of the sacredness of life. The last-named motive was also evident in the fair-certainty situation. Those who were exposed to the absolute-certainty version of the episode assigned their reluctance to a feeling that lynching would be committing murder. Supporting this view also appeared distaste for taking human life, viewing life as sacred.

For Option 3 (Non-participation). Those who, in the face of uncertain evidence, would not participate in any way were motivated by the idea that as American citizens the abductors ought to be given a fair trial by jury. Furthermore, the majority of these subjects would attempt either to persuade the crowd to abandon the attempt or to aid in some way to bolster up the officers of the law. They felt also that drastic action was contrary to American institutions as well as against their religious convictions.

In the fair-certainty situation again the majority wanted to have the suspects given a fair trial by jury as American citizens. These, too, would attempt to discourage the crowd or otherwise prevent violent action because of their ideal of law and order, their respect for legal institutions, and their distaste for taking human life. The same attitudes were evident for the group confronted with the absolute-certainty situation. Here again was an insistence upon the right of trial by jury as well as an emphasis upon that attitude toward life formed through previous training.

For Option 4 (Attempt to reason with the crowd to avert hasty action). All responses ranged high in situations of un-

certainty and fair certainty. In fact, the only response not much in evidence was in connection with the statement that the law would take its normal course and bring those guilty to justice. In other words, the individual would appeal to every possible logical argument to avert hasty action. Motives having reference to the injustice and unfairness of the impending action were frequently checked, along with the conception of law and order and the aversion to taking human life.

Where absolute evidence had been introduced, nine subjects indicated a definite attitude against the proposed action based on the terrible disadvantage in which the culprits find themselves, together with the flavor of illegality of the whole procedure and respect for the sacredness of human life.

Personal Reactions Following Initial Reaction: Social Facilitation, etc. According to their own report, subjects were affected by those about them as follows: tense breathing, 85 per cent; fear, 6 per cent; anger, 3 per cent. Six per cent were unable to report definitely.

Eighty-three per cent of the subjects felt that their election of options would not have been materially different had they been by themselves rather than with a group. Eleven per cent felt that their election might have been different; 6 per cent were noncommittal.

Only four of the subjects had ever witnessed a mob (one each in Omaha and Des Moines; two in Chicago).

None had ever participated in a mob. Invariably childhood was indicated as the time when such cases were first heard of, but most of the subjects had engaged in discussion concerning mobs on various occasions; none had, however, ever contemplated being involved in a mob at any time.

Eighty-four per cent of the subjects thought that others in the crowd felt exactly the same as they (social projection); 7 per cent similarly; and 3 per cent that some of the others felt the same.

Training and Background. Of those indicating immediate participation in the mob, seven had no definite religious affiliations, whereas six had; two others indicated a loose affiliation;

four were noncommittal. Those electing courses of action 2, 3, and 4 indicated their affiliations as follows: 98 with definite affiliations were of the opinion that to such training they might attribute their reactions; three with definite religious affiliations saw no connection between their motives to be more a matter of respect for American institutions, law and order, fair play, and clean sport.

Most of the subjects were of Dutch, English, or German extraction; all were native to Iowa or states contiguous thereto.

SECOND EXPERIMENT⁷

As discussed earlier, the Illinois study followed the general plan of the Pella study, with the additional phase of introducing a newspaper "extra" as a part of the stimulus materials. The variations were reduced to two, one a condition of absolute certainty and the other of fair certainty. There was also a pre-testing of the experimental materials by having a newsboy appear at a home where a bridge party was in progress. When the news item was concluded, one of the men, who had frequently consulted his watch during the reading, arose and left the house. He returned a few minutes later with his hat, coat, and ignition keys, and asked, "Aren't we going?"

Since this pre-test was successful with adults in carrying a surprising degree of plausibility, it was believed that the effect would be similar with the subjects used. In order to secure further evidence of the plausibility of the stimulus material from the point of view of the subjects themselves, the following question, Number 22, was placed in section 6: "At what time in the course of this experiment did you recognize the fact that the news story read by your instructor was not an account of a real kidnaping and lynching?"

⁷This study was planned in Iowa City with synchronization of events, places, and persons in the locale where the experiment took place, namely, in Bloomington and Normal, Illinois, and their immediate environs. The experimental portion of the study was carried out by Mr. Stoltz with the assistance of faculty of Illinois State Normal University.

TABLE 2

POINTS AT WHICH SUSPICION APPEARED THAT THE INCITATION WAS NOT
BONA FIDE

1. Suspected it as an experiment from beginning	5
2. Along midpoint in reading of "extra"	2
3. At completion of incitation	3
4. On appearance of analysis form	1
5. At conclusion of the experiment	8
6. When Item 22 (Supplemental Information) was reached	12
7. No suspicion at any time; implicit belief that actual event had taken place	61
8. Uncertain just when suspicion entered	5
9. Failed to indicate	24
Total	121

The fact that only five subjects were at the outset suspicious of the procedure, and only five more at the introduction of the analysis form, suggests that the procedure was surprisingly effective.

RESULTS: BLOOMINGTON-NORMAL GROUPS

TABLE 3

SUBJECTS' ABORTED RESPONSE UNDER TWO CONDITIONS OF INCITATION (N=121)

OPTION	N	FAIR CERTAINTY	ABSOLUTE CERTAINTY
1 . . . active part	9	4	5
2 . . . minor role	2	1	1
3 . . . observe	38	18	20
4 . . . avoid	45	29	16
5 . . . deter	27	15	12
Total	121	67	54

TABLE 3-A

ANALYSIS OF REACTIONS IN ACCORDANCE WITH ADVANCEMENT
IN COLLEGE TRAINING (N=121)

		OPTION				
CLASS	N	1 (ACTIVE)	2 (MINOR)	3 (OBS.)	4 (AVOID)	5 (DETER)
Fr.	42	4	1	13	15	9
So.	33	2	1	10	9	11
Jr.	25	2	0	11	8	4
Sr.	21	1	0	4	13	3

TABLE 3-B

REACTIONS BY SEX

		OPTION				
	N	1	2	3	4	5
Men	46	6	2	19	9	12
Women	75	3	0	19	36	15

As in the first experiment, the same general phenomena of differential response are observable. The number electing active participation is only a minor portion of the whole (about 12 per cent); the tendency toward participation declines with degree of certainty of guilt, whereas the disposition to deter increases. Likewise, freshmen elect active participation out of proportion to their group as compared with upper classmen. This finding also holds for men as opposed to women; approximately twice as many women as men elected to remain away.

Primary Motivation. In this study, the motivational analysis form provided for the identification of motives appropriate, respectively, for each of five optional courses of action. Detailed analyses of the first two of these responses are presented in Table 4, supplied by the ten subjects electing the participation option and including also those who would join the mob for a minor part (option 2).

TABLE 4

SUMMARY OF MOTIVATIONS UNDERLYING OPTIONS 1 AND 2

MOTIVATIONS	YES	NO	?
1. Sympathy for criminals' victim	9		1
<i>Lack of confidence in the law</i>			
2. Fear "crooked" lawyer	8	1	1
3. Feel the law is unreliable	4	4	2
4. Fear governor's pardon	8	1	1
5. Feel law uncertain and ineffective	9		1
<i>Individuals' justification of lynching</i>			
6. Protect self, others from violence	9	1	
7. Fear criminal's escape	4	4	2
8. Should be lynched as example	9	1	
<i>Submissiveness to the crowd</i>			
9. In sympathy with the crowd	5	3	2
10. Think crowd in sympathy with leaders	9	1	
11. Would follow leader's suggestions	6	2	2
12. Surrender criminals at leader's suggestion	5	2	3
<i>Moral consciousness in crowd</i>			
13. In crowd, would escape punishment	4	4	2
14. Could not punish me, alone	7	3	
15. Not possible to punish entire crowd	5	2	2
16. Many wish lynching; it must be right	7	2	1
17. So large a crowd could not be wrong	5	3	2
18. Lynching a public duty	6	2	2
19. Would be encouraged by absence of officers	1	7	2
20. Would strike criminals	3	5	2
21. This lynching is justifiable homicide	8	1	1
22. Could easily be influenced to change mind	1	7	2
<i>Additional motivation for option 2</i>			
1. Take minor part to avoid legal complication		1	
22. Would provide rope	1		

For the other three options, the general nature of the responses only is indicated below.

For Option 3 (Join the crowd to see what happened). The main considerations of these subjects appeared to be interest in the actions and reactions of the criminals, the mob, and the mob leaders, with a parallel desire to escape legal complicity. Twenty-four of the 38 subjects would remain at a safe distance, while the others felt some degree of sympathy for the mob's objective and also indicated some susceptibility to the idea of moral consciousness demonstrated in crowd prestige.

For Option 4 (Join to avoid hasty action or deter the mob if possible). The principal motivations appeared to be three-fold:

- (a) The feeling that it is wrong for the mob to take the law in hand.
- (b) The belief that lynching is murder.
- (c) The feeling that lynching would be a blot on the record of the community.

Eighteen, or almost 75 per cent, of the subjects elected this course because of their distaste for taking human life, 11 subjects indicated that they would plead with the crowd, and 19, or almost 80 per cent, said they would assist the officers against the mob.

For Option 5 (Would not join the crowd). Subjects electing this course of action did so primarily because of their aversion to lynching. Thirty-five of the 45 held lynching to be a blot on the community; 31 held lynching to be murder; and 34 indicated that the criminals were entitled to a trial by jury. This group, furthermore, indicated confidence in legal processes, although a very few felt that lynching, in some instances, could be condoned as an instance of summary justice for the protection of society.

Supplementary Data on Subjects. Most of the subjects were residents of the state of Illinois, with a scattering representation from nine other states from Ohio to California. By extraction they were predominantly English, German, and Irish, with Scotch represented rather strongly. As to church membership, all of the usual denominations were represented, the Methodists predominating. Only five indicated no affiliation. The measures of intelligence, obtained chiefly by a reading comprehension test, indicated a slight superiority for those tending to elect avoidance or deterrent roles, as opposed to those who would participate. Likewise, there was a slightly greater educational advancement of those two groups over the participators. The same situation held also for advancement in age. Other data pertaining to social facilitation, social projection, earlier contacts with lynching, and related items are summarized in Table 5.

Personality Traits and Behavior. With the expectation in mind that those subjects tending to participate might disclose greater emotional instability, greater extroversion, and possibly

TABLE 5

PERSONAL DATA AND EARLIER EXPERIENCE AND ATTITUDES RELATIVE TO MOBS^a

	I-II PART. N=10	III OBSERVE N=38	IV DETER N=25	V AVOID N=45
1... affected by others' reactions?	1	8	7	9
2... response (option) ... different if alone	0	3	2	0
3... ever witnessed mob in action?	1	8	10	6
4... discussed lynching much previously	8	18	16	18
5... ever thought of being in mob	4	13	11	9
6... definite church affiliation	7	27	18	34
7... in mob ... would act as indicated	7	26	17	24
8... account moved deeply	10	27	22	37
9... San Jose lynching justified	7	10	8	8
10... St. Joseph lynching justified	6	3	0	6

^a The negative responses are omitted, since positive response implies rejection except when, in a few instances, no response was indicated.

other traits of the pattern normally to be expected, the subjects were given the Bernreuter Personality Inventory and the Allport A-S Reaction Study for ascendance-submission. Although any conclusions would necessarily be limited by the degree of validity of the test instruments, it was deemed of interest to utilize these two tests to disclose possible differences between the different types of reaction. These results are presented in Table 6.

TABLE 6

AVERAGE SCORE OF SUBJECTS ON BERNREUTER PERSONALITY INVENTORY AND ALLPORT A-S REACTION STUDY

	BERNREUTER				ALLPORT A-S ^a
	NEUROTIC ¹	INT.-EXT. ²	DOM.-SUB. ⁴	SELF-SUF. ³	
I-II Partic.	48.20	39.80	53.40	41.90	5.70
III Observe	52.24	49.41	47.43	50.40	6.28
IV Deter	56.20	50.56	53.70	51.93	5.22
V Avoid	49.30	43.00	50.44	51.11	5.95

¹ High score=emotionally unstable.

² High score=self-sufficient

³ High score=toward introversion.

⁴ High score=increasing domination.

^a Score in deciles, 10 extreme submission, 1 extreme ascendance.

Although there is indicated, subject to the limitations of the instruments, slightly lesser neurotic tendencies in subjects electing participation than avoidance, the differences are not great enough to have statistical significance. The participators likewise show a tendency to be less self-sufficient than those electing other courses. The participator group shows its greatest difference in a definitely higher degree of extroversion. The standard error of the difference, however, between this average and that for the deterrent class shows that the difference between the two averages, divided by the standard error of the difference, is 1.05, indicating the chances to be 85 in 100 of there being a true difference greater than zero. While there is a slight tendency also indicated toward dominance, the differences have no statistical significance. The Allport results tend to confirm the dominance-submission scale of the Bernreuter test.

DISCUSSION

Shortly before the first of these studies was made, there occurred in Iowa three incidents which amplify existing knowledge of crowd behavior. These were: (1) the LeMars mortgage foreclosure riot, in which a judge was taken from his bench by a mob and driven by automobile outside the community under threat of hanging if he did not take into account mitigating circumstances applicable to farmers who were destined to be victims of impending dispossession; (2) the Dennison riots, at which a public dispossession sale was disrupted and deputies resisted by irate "buyers"; and (3) the foreclosure sale near Pella, which terminated in an arbitration meeting.

The latter began as a "penny-sale" conspiracy,⁹ following the pattern then in vogue, but it became known that the mortgage-holder himself would be facing bankruptcy in the event that nothing was realized from the sale. The more reasonable ele-

⁹ An expedient arising during the wave of foreclosures in the mid-west, to circumvent actual dispossession. Friends of the farm owner would conspire to bid in with small amounts (1c to 50c usually), with the tacit understanding that the horse or cultivator or plow thus purchased would be held in trust for the original owner.

ments in the crowd then began a series of discussions which terminated in a re-financing of the farm on a basis of mutual concessions.¹⁰

As consequences of the frustration of egoistic drives and continued deprivation, the LeMars and Dennison incidents are clearly examples of conflict between long-established attitudes toward legal processes and threatened extinction of their accustomed opportunity for livelihood. The incidents themselves represent a climax in shift of attitudes to the point where impending consequences temporarily overshadow usual regard for normal procedures and due process of law. Individually, no one would dare oppose openly the edict of the court, but at this time repetition of the threat to subsistence carried to more and more people the belief that they, in turn, might tomorrow become victims unless amelioration of the conditions were sought through direct group action in the form of threats or blocking of court orders. Hence the circumventions, near-violence, and all-but-actual bloodshed. The refusal of the Governor to permit the National Guard a free hand, and the restraint exercised by both officials and the crowd itself, amply support the thesis that in the crowd the individual acts in accordance with attitudes earlier acquired, which at the time remain as deep-seated facilitators or inhibitors of any action that may then be forthcoming, and *are subject to variation*. Such incidents also demonstrate that knowledge of the constituency of any aggregation provides insight into proper means of dealing with the participants, should crowd feeling develop. The three episodes may be regarded as field studies of the same type of individual used in the two experiments, and as conforming to the same behavioral principle. (Cf. 7; 9.)

SUMMARY AND CONCLUSIONS

In view of the extreme difficulties in experimental study of mob behavior, recourse was had to a modification of experimental procedure closely approximating mob incitation (assemblage, episode, leader), with provision made for recording at the height

¹⁰ This incident was reported by Mr. Mennenga.

of excitement the reactions and motivating context of the individuals present.

The procedure required three antecedent preparations: (a) the planning in every detail of a complete mob-inciting, simulated crime, finally reduced to news-bulletin or news "extra" form; (b) preparation for the leader role; and (c) a comprehensive study of all usually present motives and incentives likely to function in any individual present, with reduction of the lists to four or five categories of response. The success of the technique was hence dependent upon the plausibility of the "crime" presentation, the duration of the emotional state through the recording period, and the fidelity of the reactions.

For purposes of testing the susceptibility of the individual to variations in the incitation, the stimulus material was varied for different groups. In one instance the evidence of guilt was circumstantial only; in two, partly circumstantial; in two, absolute.

Conclusions can be drawn only for such types of subjects as were used, for areas as the central Mississippi valley, and for such reactions as are permitted in the procedure. As such, the findings are of considerable value, nevertheless, since other more violent and drastic manifestations of individual behavior in the crowd situation would differ mostly in degree, in the proportion of individuals electing the various options, and in the variable degrees of motivation: the *patterns of response* would probably not deviate greatly.

1. By the pseudo-crime method of incitation, a situation was actually created that was assumed as genuine by all but 10 out of 97 subjects, with 24 other cases uncertain (Illinois subjects).

2. Under the stress of the incitation (all groups), 12 per cent, or roughly 1 in 8, indicated an inclination to join the mob forthwith with expectations of actual participation. Under conditions of uncertainty as to guilt, only 2 appeared; whereas under "fair certainty" the number rose to 8; and under "absolute certainty," the number jumped to 20.

3. In all groups, those who would go along to see what

happened numbered 57 (23 per cent of the total), without much variation with degree of guilt.

4. Seventy-two (29 per cent) would have gone with the intention of deterring the mob from lynching the victims. Of these, 20 emerged in response to the "uncertainty" incitation (Pella only); 31 to "fair certainty" (both); and 21 to "absolute certainty" (both). It is noted that in the Pella groups the number under "uncertainty" (20) dwindled to 9 under "absolute certainty."

5. Eighty-six, or 35 per cent, would have chosen to remain away entirely. In this category women predominate.

6. From (2) and (4) above, it may be generalized that participation tends to increase with certainty of guilt, while deterrence tends to decrease.

7. Analyses of motivation tend to establish the ascendance of past experience, particularly attitudes toward administration of justice and attitudes reflecting earlier religious training. The latter appears as the dominant drive toward *deterrence* of mob action. Of those electing immediate participation (first experiment), about half had no church affiliation, whereas nearly all of those assuming non-participant or deterrence roles had affiliation of some kind.

8. Participants disclosed a definitely greater degree of extroversion, approaching statistical validity, and less self-sufficiency, than non-participants. Other differences were present but not marked. Non-participants disclosed generally median degrees of introversion-extroversion; those who would go along merely to see what happened rated highest in submissiveness.

9. A somewhat well-defined tendency toward greater conservatism with age and advancement in academic training was disclosed. Freshmen, out of proportion to their numerical strength, tended toward participation, as compared with upper classmen. Similarly, women tended toward a greater conservatism than men, approximately twice as many as men electing non-participant roles. This observation holds for both experiments, in which, taken together, the sexes were represented almost equally.

10. In general, the two studies support the thesis that in the crowd setting the individual will behave in accord with the dominance of previously established habits, attitudes, and behavior patterns, but that the action itself will be to some degree conditioned by the nature of the situation, since the response of participation or deterrence will be to some degree in accordance with the degree to which guilt is or is not completely established.

BIBLIOGRAPHY

1. ALLPORT, F. *Social psychology*. Boston: Houghton Mifflin, 1924, Ch. XII.
2. *The Call-Bulletin*, San Francisco, Nov. 27, 1933. Extra edition, account of the San Jose lynching.
3. *The Chicago Daily Tribune*, Chicago, Nov. 27-Dec. 12, 1933. Accounts and comments on San Jose and St. Joseph lynchings.
4. CLARK, H. The crowd. *Psychol. Monogr.*, 1916, 21, 26-36.
5. *The Kansas City Journal-Post*, Kansas City, Mo., Nov. 29-Dec. 30, 1933. Account and discussion of the St. Joseph lynching.
6. LEBON, G. *The crowd*. London: Unwin. Transl. 1896.
7. MARTIN, E. S. *The behavior of crowds*. New York: Harper, 1920.
8. MEIER, NORMAN C. Motives in voting: A study in public opinion. *Amer. J. Sociol.*, 1925, 31, 199-212.
9. TROTTER, W. *Instincts of the herd in peace and war*. London: Unwin, 1923.

TRAINING IN DEMOCRATIC LEADERSHIP *

ALEX BAVELAS AND KURT LEWIN

University of Iowa

This is a preliminary report about a rapid retraining of mediocre leaders into efficient democratic leaders.¹

Good leadership is recognized as one of the outstanding conditions in any field of group life or cooperative endeavor. Large organizations, such as the W.P.A., Boy Scouts, Y.M.C.A., school systems, factory organizations, all require leadership for the organization as a whole (head-leader), and leadership for the smaller groups which actually make up the body of that organization (sub-leader). We shall speak here of the latter type of leadership, although we believe that the former does not present fundamentally different problems.

In regard to the head-leader, it is essential for an organization to get the best person available. In regard to the hundreds of sub-leaders working in a large organization it is one of the major considerations to eliminate the inefficiency caused by the poorer leaders because they account for a disproportionate amount of the trouble and avoidable expense.

Poor leadership can be eliminated either by careful selection and, if necessary, dismissal of personnel, or by training. The difficulty of predicting leadership ability is known to be great. Dismissal involves much waste and expense. In recognition of this situation, training of leaders has been widely attempted. However, frequently it has not been very satisfactory. Also,

* A cooperative study of the Child Welfare Research Station, University of Iowa, the W.P.A. of the State of Iowa, and the Jewish Community Center, Des Moines.

¹ For a more detailed account, see Bavelas, A. Morale and leadership training. In *Yearbook of the Society for the psychological study of social issues*. (In press.)

there is no actual scientific knowledge about either the percentage of poor leaders that can be improved by training or how far the improvement can go.

I. EXPERIMENTAL SET-UP

As a first step in studying scientifically the possibilities of leader re-training, the following experiment tried to test under controlled conditions the efficiency of certain training methods for a particular field: recreation. The leaders were picked so that their age and habits of long standing should present particularly difficult cases for retraining.

The experiment was conducted in a summer "Home Camp" (at the Jewish Community Center, a non-sectarian service) whose children (mainly lower middle-class) were free to attend from day to day. Among the W.P.A. recreation leaders who worked on this project, six were selected by their supervisors as definitely unsatisfactory — four women leading handcraft classes and two men leading outdoor games. They were between thirty-five and forty-five years of age, and had been doing recreational work on the W.P.A. for an average of three years.

The experiment proceeded in the following manner: (1) All leaders were tested by observing and quantitatively recording their actual behavior "on the job." This included the way they dealt with the children and the resulting behavior of the children. (2) The subjects were divided into a "training group" and a "control group" which was not trained. Each leader in the training group had his counterpart in the control group. (3) The "training group" was then trained for three weeks (twelve days) for not more than two hours on each day. During these three weeks, both the training and the control group continued their work at the recreation center. (4) At the fourth week, both the trained and the non-trained leaders were tested again "on the job" by the same methods as at the beginning of the experiment.

II. RESULTS

A. The Leaders' Behavior before Training

1. *Behavior with children.* The treatment of the children was not unfriendly and sometimes the leaders showed a measure of personal involvement. Every leader was relatively well-trained in his particular field — flower making, clay modeling, playground games.

Figures 1a and 2a present the quantitative data about the way the leader controlled the children.

Leaders A and B (Figure 1a), who did playground work, controlled the children predominantly (60 per cent) by direct "leader-initiated commands." The somewhat milder form of direct control which consisted of giving commands after having

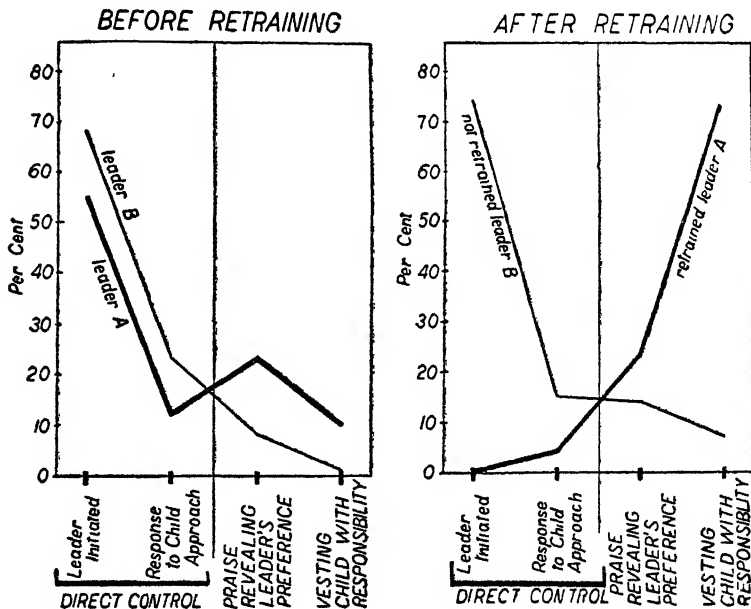


FIG. 1A

FIG. 1B

RETRAINING OF LEADER A

The frequency with which leader A uses authoritarian methods of direct control drops as a result of retraining from 77 per cent to 4 per cent. Instead, he uses a democratic, initiative-stimulating method, the frequency of which has risen to 73 per cent.

been "approached by the child" occurred relatively infrequently (16 per cent). Less dominating and more evocative than these two "direct" methods of control is the guiding of the child by praising certain behavior or by making the leaders' own preferences known. A and B used this method seldom (12 per cent). The democratic, initiative-stimulating method of placing the responsibility of a "wise" choice in the children themselves was practically never used (5 per cent).

In summary, before training, leaders A and B used the authorization methods of direct control in about 80% of their action.

The leaders C and D show a similar predominance (66 per cent) of "direct" methods of control (Figure 2a).

Having to do with handcraft rather than with the traditionally "tougher" play-ground activities, they used mainly the somewhat milder authoritarian form of giving commands after

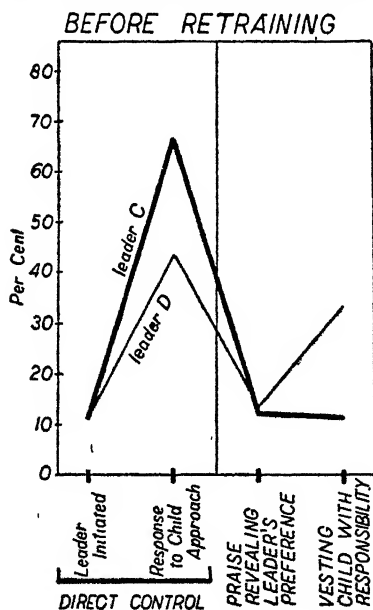


FIG. 2A

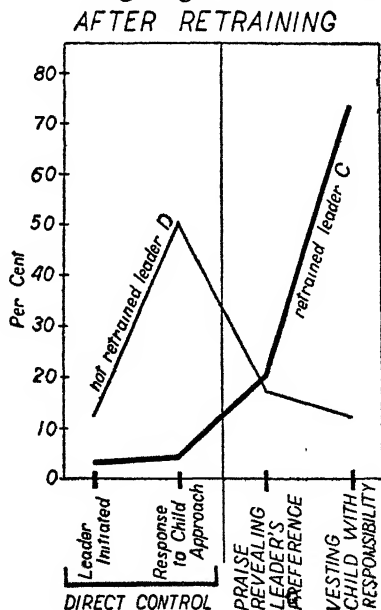


FIG. 2B

RETRAINING OF LEADER C

The frequency with which leader C uses authoritarian method of direct control drops as a result of retraining from 77 per cent to 7 per cent. Instead, he uses a democratic, initiative-stimulating method, the frequency of which rises to 73 per cent.

they were approached. Directing the group by praise and by vesting the children with responsibility occurred in 12 per cent and 22 per cent respectively.

The lack of democratic techniques was apparent also in the *methods of teaching* the craft or game (our film record of leader and group shows this clearly). The leader saw to it that every child had to follow the same uniform pattern of work. The productions of the children were supervised in minute steps. When a child did not succeed, the leader would help him, usually by doing the operation himself.

2. *Morale of the leaders.* There was every indication that the morale of the leaders was low. In fact, most of them disliked their work, felt very insecure, and were suspicious of the organization. They openly stated that for everyone they knew this work was drudgery to be done just well enough to keep the job. The facial expression and bodily postures of these leaders while they worked indicated a mixture of apathy, worry, and unhappiness (see films).

B. The Leaders' Behavior after Training

1. *Behavior with children.* Figure 1b shows the *methods of control* used by leaders A and B at the end of the experiment. Leader A had three weeks retraining, leader B had no retraining. B's methods have not changed or have become a bit worse. The frequency with which the retrained leader A uses authoritarian direct control methods has dropped from 77 per cent before training to 4 per cent after training. Instead, he uses a democratic initiative-stimulating method (73 per cent).

A similar shift occurred in the retrained leader C as compared with the non-trained leader D (Figure 2b). D's methods have not changed or have become a bit worse. The frequency with which the retrained leader C uses direct commands has dropped from 77 per cent before training to 7 per cent after training. Instead, he uses an initiative-stimulating method (73 per cent).

The *teaching methods* of the non-retrained leaders did not show any indication of change. The retrained leaders shifted

from "class-room" techniques characterized by dependence of the children and by uniformity of procedure to "group methods" which created productivity and co-operation. The success of these group methods was evident in (a) a doubling of the number of children attracted to participation, (b) the enthusiasm and persistence of the group, (c) the efficiency of work organization, (d) the high degree of self-discipline, and (e) the quality and output of the work.

2. *Morale of the leaders.* One of the most salient results of the retraining was the change from a definitely low to a definitely high morale. Leaders who had never been observed to smile at their work "loosened up" after the first week. After three weeks, the "drudgery" has turned into a meaningful and challenging job whose values for the child and for society at large were keenly felt. There was striking evidence of initiative in trying to find non-relief work and a readiness to tackle major difficulties.

The change to an all-around high morale of the leaders was reflected in similar changes of morale among the children. The recreational activities which previously were seen merely as a source of individual pleasure became transformed by the children themselves into worthwhile, socially oriented, and long-range projects. The scope of the group's activities and the efficient self-organization of the clubs far surpassed the levels achieved under the previous circumstances.

III. METHODS OF RETRAINING THE LEADERS

In essence, the method of retraining was a combination of changing the attitudes of the leaders and changing their techniques. Neither alone would have sufficed. These changes were achieved by a type of "clinic-on-the-job," and by applying genuinely democratic methods to the procedure of training.

We did not attempt to bring about these changes by "talks" about democracy, since every one of the trainees had previously gone through a standard training procedure and was accustomed to "talk" about recreational methods in democratic lingo. A

first step was to make the trainees more sensitive to the multitude of ways in which a leader can meet the various social situations. This was done by observing many leaders — good and poor, by observing each other, by observing the trainer himself, and by studying films from experiments about democratic, autocratic, and laissez-faire types of leadership. This led to an amplification of the objectives of recreational work. The various unrelated and vague goals (such as teaching skills, providing fun for the children, keeping discipline) became connected and part of a more far-reaching and, at the same time, more concrete goal. In the light of this new, organized goal system, the techniques became integrated among themselves and with the underlying attitudes.

Three points added greatly to the success of the training: (1) In spite of the new and added responsibility inherent in democratic leadership, the trainees felt keenly their own greater calm and poise, after they discovered that group discipline no longer depended upon their constant vigilance. (2) The trainees felt quickly the good effect which the turn toward democratic group methods had on the children. (3) The general belief of the trainees in the democratic procedures was strengthened by their own experience as members of the thoroughly democratic training group.

This first experiment is now being followed up by the training of people who will go out to train or retrain W.P.A. leaders in various fields. This will permit the testing of the efficiency of these training methods on a broad scale.

THE 100 GREATEST BOOKS SELECTED BY 100 QUALIFIED PERSONS

DANIEL STARCH

Daniel Starch and Staff, New York City

The purpose of this inquiry was to determine what, in the judgment of competent persons, are the greatest books of all time. Numerous lists of "great" books, or "best" books, or books "everybody should read" have been prepared ever since there have been books. The Bible, itself, is not a single book but a collection of the most important religious books or scriptures selected by consensus of judgments of early church authorities from among works that had been written during a thousand years prior to about 100 A.D. Most lists have been prepared by individuals either alone or with the assistance of a few others for special purposes or from specific points of view—such as books we like, or books an educated person should read, or leading books published since a certain date, and the like.

Perhaps the two most important lists of great books assembled in recent times were the one compiled by the English naturalist and statesman, Sir John Lubbock, and the other begun by John Erskine, author and professor at Columbia University.

Sir John Lubbock published, in the *Pall Mall Gazette* on January 11, 1886, what he considered to be the 100 best books—books that were "necessary for a liberal education" or "most conducive to a healthy mind." He "excluded (1) works by living authors, (2) science, and (3) history, with a very few exceptions," which he mentioned rather in their literary aspects. The list was published also in the *Contemporary Review*. Various

persons were then asked to comment on this list and make suggestions. Comments and suggestions were received from James Bryce, John Ruskin, William E. Gladstone, Thomas Carlyle, Joseph Chamberlain, the then Prince of Wales who became King Edward VII, and others. Most of them approved Lubbock's list or suggested a few additions or omissions. Ruskin criticized it violently, crossing off about half of Lubbock's books including such important works as Aristotle's "Ethics," Marcus Aurelius' "Meditations," Confucius' "Analects," Smith's "Wealth of Nations," Sophocles' "Oedipus Rex," Euripides' "Medea," Darwin's "Origin of Species," and Goethe's "Faust."

In the 1920's, Dr. John Erskine composed a list of sixty to seventy books as his choice of "the great works of Western culture." These books were the substance of a two-year reading course at Columbia University. Later, the list was modified and extended by members of the faculty of Columbia University and published by the American Library Association. The Erskine-Columbia list was further revised and extended by Professor Mortimer Adler at the University of Chicago and by the faculty of St. John's College at Annapolis, where it became the core of its course of study. This resulted in a list of slightly more than 100 books and is published in Adler's *How to Read a Book* (1939).

So far as I am aware, no one has compiled a list of the world's great books by consensus of a large number of competent persons in accordance with a definite statistical procedure. Hence, I undertook, in 1937, to compile a list on such a basis. The procedure was as follows:

Four persons, broadly familiar with literature and the various fields of knowledge, made a list of all books which conceivably might be considered by competent persons to be counted among the 100 greatest books. No books published since 1900 were included. A generation or more must elapse before the permanent place of a book can be judged. This produced a total of 281 books. There were then arranged in alphabetical order and printed in mimeographed form. This list of 281 books was then submitted successively to judges with the request:

"Please make a check in front of each book that you would include among the first thirty-five great works and among these double check ten of the really greatest books. Please add at the end of the list the names of any others which you think should be included among the first thirty-five and then check them according to your opinion of their greatness. You are to regard greatness whatever you think it should mean. It should include, however, influence on mankind and civilization of an enduring character."

This procedure was continued until a total of one hundred judges had made their choices. The persons who were asked to make the selections included many distinguished authorities in the various fields of knowledge and literature. Approximately half of these one hundred persons were literary critics or professors of language and literature. The remaining half included persons distinguished in the various fields of knowledge—science, philosophy, economics, history, and government.

The returns were then compiled as follows: A weight of three and one-half was given to each book with a double check, and a weight of one to each book with a single check. The composite judgment yielded the following list of books with their respective scores:

THE 100 GREATEST BOOKS
*Copyright, 1937, by Daniel Starch **

Author	Title and Date	Consensus of Judgment Score
1. Numerous authors	The Bible—450 B.C.-200 A.D.	309
2. Shakespeare, W.	Hamlet—About 1600	211
3. Aristotle	Works—About 350-325 B.C.	198
4. Homer	Iliad—About 800 B.C.	197
5. Darwin, C.	Origin of Species—1859	187
6. Dante, A.	The Divine Comedy—About 1300	173
7. Plato	Republic—About 400-350 B.C.	166
8. Goethe, J. W.	Faust—1808	156
9. Confucius	Confucian Classics—About 525-480 B.C.	147
10. Milton, J.	Paradise Lost—1667	120
11. Cervantes, M.	Don Quixote—1605	119
12. Mohammed	Koran—About 610-632	111
13. Chaucer, G.	Canterbury Tales—1386	97
14. Newton, I.	Principia Mathematica—1687	89

Author	Title and Date	Consensus of Judgment Score
15. Kant, I.	Critique of Pure Reason—1781	88
16. Virgil	Aeneid—About 70-19 B.C.	84
17. Tolstoy, L. N.	War and Peace—1866	83
18. Marx, K.	Das Kapital—1867	80
19. Homer	Odyssey—About 600-500 B.C.	73
20. Bunyan, J.	Pilgrim's Progress—1678	72
21. Gibbon, E.	The Decline and Fall of the Roman Empire—1772	69
22. Bacon, F.	Novum Organum—1612	66
23. Sophocles	Oedipus Rex—About 468-406 B.C.	63
24. Buddha	Buddhist Suttas—About 480 B.C.	62
25. Smith, A.	The Wealth of Nations—1776	58
26. Aesop	Fables—About 570 B.C.	50
27. Montaigne, M.	Essays—1580	50
28. Hugo, V.	Les Misérables—1862	49
29. Plato	Phaedo—About 400-350 B.C.	49
30. Mendel, J. G.	Principles of Heredity—About 1850-1884	48
31. Burton Translation	Arabian Nights—1885	44
32. Shakespeare, W.	King Lear—About 1600	43
33. Aquinas, St. T.	Summa Theologiae—1265	42
34. Aeschylus	Prometheus Bound—About 500-456 B.C.	41
35. Hindu Sources	Bhagavad-Gita—About 200 A.D.	40
36. Calvin, J.	The Institutes of the Christian Religion—1536	40
37. Rousseau, J. J.	Social Contract—1762	39
38. Plutarch	Parallel Lives—About 100-120 A.D.	37
39. Blackstone, W.	Commentaries on the Laws of England—1765	36
40. Ancient Service Books	Book of Common Prayer—About 1500	36
41. Boswell, J.	The Life of Samuel Johnson—1791	34
42. Defoe, D.	Robinson Crusoe—1719	33
43. Shakespeare, W.	Macbeth—About 1600	33
44. Marcus Aurelius	Meditations—121-180	32
45. Emerson, R. W.	Essays—1841	32
46. Sophocles	Antigone—About 468-406 B.C.	32
47. Shakespeare, W.	Romeo and Juliet—About 1600	31
48. Thucydides	History of the Peloponnesian War—About 420 B.C.	31
49. Dickens, C.	David Copperfield—1850	30
50. Franklin, B.	Autobiography—1784	30
51. Locke, J.	Essay Concerning Human Understanding—1690	29
52. Bryce, J.	The American Commonwealth—1888	28
53. Herodotus	History—About 490-480 B.C.	28
54. St. Augustine	Confessions—399	28
55. Whitman, W.	Leaves of Grass—1855	27
56. Caesar, J.	Commentaries on the Gallic Wars—About 50 B.C.	27

Author	Title and Date	Consensus of Judgment Score
57. Stowe, H. B.	Uncle Tom's Cabin—1852	27
58. Lincoln, A.	Addresses—1858-1865	26
59. Malthus, T. R.	On the Principles of Population— 1798	26
60. Galileo Galilei	Dialogue of the Two Chief Systems —1632	26
61. Omar Khayyam	The Rubaiyat (Fitzgerald)—1859	26
62. Rabelais, F.	Gargantua and Pantagruel—1533	25
63. Shelley, P. B.	Poems—About 1820	25
64. Machiavelli	The Prince—About 1512	24
65. Descartes, R.	Discourse on Method—1636	23
66. Fielding, H.	Tom Jones—1749	23
67. Hume, D.	An Enquiry Concerning Human Understanding—1748	23
68. Spinoza, B.	Ethics—1675	23
69. Thackeray, W. M.	Vanity Fair—1846	23
70. Voltaire	Candide—1759	23
71. Wordsworth, W.	Poems—1807	23
72. Rousseau, J. J.	Confessions—About 1770	23
73. Twain, M.	Tom Sawyer—1875	23
74. Dostoevski, F. M.	Crime and Punishment—1866	22
75. Epictetus	Discourses—About 90 A.D.	22
76. Harvey, W.	On the Motion of the Heart and Blood—1628	22
77. Euripides	Hippolytus—About 480-406 B.C.	21
78. Boccaccio, G.	The Decameron—1348	20
79. Kempis, T.	The Imitation of Christ—1471	20
80. Erasmus, D.	The Praise of Folly—About 1510	19
81. Swift, J.	Gulliver's Travels—1720	19
82. Shakespeare, W.	A Midsummer Night's Dream—1595	19
83. Faraday, M.	Scientific Papers—About 1833-1867	19
84. James, W.	Principles of Psychology—1890	19
85. Ibsen, H. J.	Doll's House—About 1880	18
86. German Sources	Nibelungenlied—About 1200	18
87. Shakespeare, W.	Merchant of Venice—1596	18
88. Pepys, S.	The Diary of Samuel Pepys—1660- 1669	18
89. Aeschylus	Agamemnon Trilogy—About 500- 456 B.C.	17
90. Bacon, F.	Advancement of Learning—1605	17
91. Burns, R.	Poems—About 1787	17
92. Macaulay, T. B.	The History of England—1848-1855	17
93. Keats, J.	Shorter Poems—1817	17
94. Shakespeare, W.	Sonnets—1609	17
95. Dostoevski, F. M.	Brothers Karabazov—1880	16
96. Tennyson, A.	In Memoriam—About 1833-1850	16
97. Carroll, L.	Alice in Wonderland—1865	15
98. Cicero, M. T.	Orations—About 80-43 B.C.	15
99. Spencer, H.	First Principles—1862	15
100. Tolstoy, L. N.	Anna Karenina—1877	15

If the writings of Shakespear are counted as one book the next six books to total one hundred are as follows:

Author	Title and Date	Consensus of Judgment Score
101. Paine, T.	The Rights of Man	14
102. St. Augustine	The City of God	14
103. Veblen, T.	The Theory of the Leisure Class	14
104. Lucretius	On the Nature of Things	14
105. Schopenhauer, A.	The World as Will and Idea	14
106. Vedic Sources	Upanishads—About 500 B C.	14

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VALIDITY OF LIST

The validity of this list, I believe, is high, in the sense that another group of 100 competent persons at the present time would choose the same list with only minor differences among the lowest ten or fifteen books. As a matter of fact, the composite selection of the first fifty and of the second fifty of the 100 persons were practically identical. As a further corroboration, I asked one hundred librarians of large libraries, and also seventy-five recent college graduates engaged in business who had been out of college five to ten years, to make their selections. The results from both groups showed substantial agreement. Possibly a dozen books at the lower end of the list were different.

The final list as here presented may therefore be regarded as *the* one hundred great or greatest books, with some reservation as to the last ten or fifteen books. It has a reliability not possessed by any previous list for the following reasons: (1) It is the fruit of the combined judgment of a highly competent group of judges. (2) These judges included representative persons, recognized as authorities, from all main fields of knowledge. (3) The judgments were expressed and collected according to a specific statistical procedure.

HOW MANY GREAT BOOKS ARE THERE?

We can answer this question from our statistical data with some assurance. If we plot the scores for the one hundred books in decreasing order from left to right, as in Chart I, we note that the first group of books stands out far above the rest. The

curve starts at a high point, drops sharply, and then pursues a slowly declining course. It appears, therefore, that there not one hundred equally great or nearly equally great books, nor

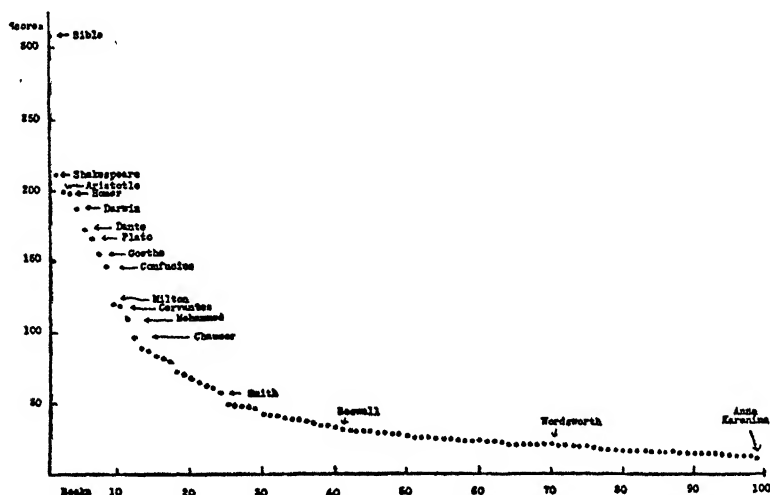


CHART I. Scores of the 100 books in order of decreasing size.

seventy-five, nor fifty, but only about twenty-five greatest books.

The chart indicates broadly that there are four groups of books:

First, the Bible. This stands out by itself as the book of books, with a score nearly 100 points or 50 per cent above the second book in the list, namely, the works of Shakespeare.

Second, the eight books, comprising the works of Shakespeare, Aristotle, Homer, Darwin, Dante, Plato, Goethe, and Confucius. These are, after the Bible, the really towering monuments of literary and intellectual history which have had the most enduring influence on mankind and civilization.

Third, the sixteen books in the list from Milton to Adam Smith. These constitute the next group of great books. There is a drop of twenty-seven points from Confucius to Milton. Possibly this is not significant and these sixteen books should not be considered separately but in a continuous series with the eight in the preceding group.

Fourth, the remaining seventy-five books are nearly on the same level. The drop from Aesop, the twenty-sixth book, to Anna Karenina, the one hundredth book, is only thirty-five points, whereas the drop from Shakespeare to Adam Smith is 161 points.

These groupings may be somewhat arbitrary, but it is evident that there are only about twenty-five works which tower above all others. These twenty-five are also in Lubbock's list or the Erskine-St. Johns' list, or both. Eighty-one of the total one hundred books are also on the Lubbock or the Erskine-St. Johns' list, or both. It must be remembered that Lubbock's list excluded, with few exceptions, books on science and history and the Erskine list excluded books of the Orient.

WHEN WERE THE GREAT BOOKS WRITTEN?

If we arrange the books chronologically by centuries, as in Chart II, we note the interesting fact that there have been two fertile periods in the world's history. The first period was the Greek-Roman-Early Christian era of about a thousand years ending around 200 A.D. The second is the renaissance era beginning about 1300 A.D. and continuing up to the present. Between these two eras there were a thousand years in which only five of the great books were produced. In fact, there were five hundred years between 600 and 1100 A.D. during which not a single work was created which was rated among the 100 great ones. Of the twenty-five greatest books at the top of the list, only one, the Koran, was produced during the eleven centuries from 200 A.D. to 1300 A.D., whereas the millennium preceding 200 A.D. produced nine of the twenty-five greatest books. The Dark Ages obviously were dark from a literary point of view.

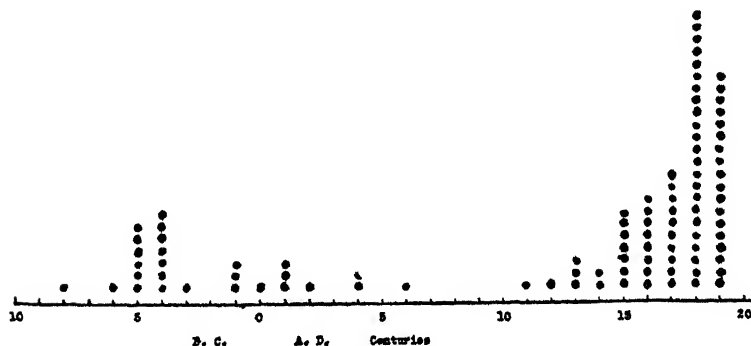


CHART II. Centuries in which each of the 100 books was written

BOOKS READ BY COLLEGE GRADUATES

This list of 100 books was submitted to 142 seniors at the end of the year in the arts colleges at Stanford University, the University of Wisconsin, and the University of Florida, with the request that each one designate with the letter R, those books of which they had read substantial portions and, with the letter F, those books which they had not read but with which they had considerable familiarity through other sources, such as reading or lectures.

The average number of the one hundred books read by these college seniors was twenty-four. The average number of additional books with which they indicated familiarity was sixteen, making a total of forty. Shakespeare's works had been read by the largest proportion, namely, 96 per cent. The Bible had been read by 67 per cent with an additional 20 per cent expressing familiarity with it. At the bottom of the list were Newton's *Principia Mathematica* and Marcus Aurelius's *Meditations*, which had been read by, or were familiar to, less than 5 per cent of college seniors.

SUMMARY

1. A method has been employed which has yielded a list of 100 great books possessing a higher degree of validity than previous lists.
2. There are only about twenty-five "greatest" books.
3. The two productive eras were the period of a thousand years ending at 200 A.D. and the period from 1300 A.D. to the present.
4. College seniors have read on the average twenty-four of these 100 books and possess familiarity with the contents of sixteen additional books.

NOTE: I wish to convey my cordial appreciation to the 100 persons whose combined judgments went into the making of the list of 100 great books. The following kindly permitted me to mention their names. (The word university or college is omitted from the names of institutions given in parenthesis):

Professors of English or Literature—Walter Barnes (New York), Philo Buck (Wisconsin), O. J. Campbell (Columbia), Hardin Craig (Stanford), Edward H. Gardner (formerly Wisconsin), W. H. F. La-

mont (Rutgers), John Livingston Lowes (Harvard), R. L. Lyman (Chicago), Stuart Robertson (Temple), Howard J. Savage (Bryn Mawr), Homer A. Watt (New York), Karl Young (Yale).

Professors of other languages, ancient and modern—Barry Cerf (Reed), J. P. W. Crawford (Pennsylvania), H. C. Lancaster (Johns Hopkins), Kenneth McKenzie (Princeton), E. K. Rand (Harvard), David M. Robinson (Johns Hopkins), Albert Schinz (Pennsylvania), B. L. Ullman (Chicago).

Authors, literary editors and critics—Robert Barlow, Clye Beck, Charlotte Becker, Grosvenor Blake, Van Wyck Brooks, Struthers Burt, Elrick B. Davis, Edwin F. Edgett, Max J. Herzberg, Chas. P. Johnson, Kent Knowlton, Sterling North, Paul Jordan Smith.

Journalists and professors of journalism—Frank Luther Mott (Iowa), Walter B. Pitkin (Columbia), William Allen White, Thomas F. Woodlock.

Professors of philosophy—John Dewey (Columbia), Will Durant (formerly California), G. T. W. Patrick (Iowa), Ralph Barton Perry (Harvard), Frank C. Sharp (Wisconsin), Edwin D. Starbuck (So. California).

Professors of history and government—Sidney B. Fay (Harvard), G. S. Ford (Minnesota), Roger Howson (Columbia), H. F. MacNair (Chicago), William B. Munro (Calif. Inst. of Tech.), Frederic A. Ogg (Wisconsin), Winfred T. Root (Iowa), Louis M. Sears (Purdue).

Professors of economics and sociology—Emory S. Bogardus (So. California), T. N. Carver (Harvard), F. Stuart Chapin (Minnesota), Charles F. Ellwood (Duke), Richard T. Ely (formerly Northwestern), Irving Fisher (Yale), Herbert A. Miller (Bryn Mawr), Robert E. Park (Chicago), E. A. Ross (Wisconsin).

Professors of various sciences—F. E. Bolton (Washington), W. W. Charters (Ohio State), Marian M. Cobb (New York Schools), Dexter S. Kimball (Cornell), Robert A. Millikan (Calif. Inst. Tech.), Frank Schlesinger (Yale), Harlow Shapley (Harvard), T. M. Simpson (Florida), Lewis M. Terman (Stanford), E. L. Thorndike (Columbia), R. S. Woodworth (Columbia).

A CASE STUDY IN THE PERPETUATION OF ERROR

GEORGE W. HARTMANN

Department of Psychology
Harvard University

Almost every literate adult within the English-speaking world has probably heard the story of the Black Hole of Calcutta, a minor event supposed to have happened almost two centuries ago in connection with the British conquest of India. Innumerable histories, major and minor, record the short but horrible tale; sober encyclopaedias give it the respectable stamp of their authority; and more recently, solid medical, engineering, and psychological textbooks have by their repeated references thereto all but universalized an awareness of this episode. One can hardly encounter any lay or professional discussion of atmospheric or ventilation problems which does not compulsively mention the Black Hole, as though no account would be complete without this scholarly adornment.

Yet there is grave doubt whether this alleged event ever actually occurred. Strangely enough — or is it strange? — psychologists have been as gullible as others in their uncritical reception and transmission of this tradition. This circumstance makes it a choice candidate for inclusion within any mature study of the mechanism of rumor. Let us examine some of the evidence, with an advance warning that full verification would be equivalent to a doctoral thesis, which the reader shall be spared.

Two things immediately strike any investigator attempting to check this incident: (1) the paucity of "eye-witness" accounts, and (2) the nationalistic alignments among historians,

the English-speaking writers generally accepting the tale without criticism, while Indian authors, regardless of faith, consider it either a pure fabrication or a gross exaggeration or distortion.

The basic document was written by John Zephaniah Holwell, who was born in Dublin in 1711 and came to Calcutta as a surgeon's mate in 1732. His grandfather was a surveyor and astrologer who issued an edition of the famous prophecies of Nostradamus. The entry in the *Dictionary of National Biography* may be consulted for further details.

Holwell became a member of the local council in the English settlement and a magistrate during the time of the disturbances between the native ruler of Bengal and the British trading community. He was captured in the storming of Fort William, spent a night confined in the Black Hole, was taken inland for a few weeks, and then released. About a half-year later, he wrote his story "From on board the *Syren*-sloop, the 28th of February, 1757." It is in the form of a letter to a William Davis, and is preceded by a "To the Reader" statement explaining why the epistle was being made public. The title page bears this quaint language, typical of the period:

A Genuine Narrative of the Deplorable Deaths of the English Gentlemen, and Others, Who were Suffocated in the Black-Hole in Fort-William at Calcutta, in the Kingdom of Bengal; in the Night Succeeding the Twentieth Day of June, 1756.

In a Letter to a Friend

By J. Z. Holwell, Esq.

London: Printed for A. Millar in the Strand, MDCCLVIII (pr. 1 sh.)

Holwell apparently considered this pamphlet a part of his general defense of his own public colonial career, for it was one of five reprinted together in a book called *India Tracts*, London, 1764. This second edition is illustrated with a frontispiece engraving of a commemorative monument in honor of the victims of the tragedy erected by him at his personal expense in 1758-9 when he served as Governor of Bengal. Holwell had his portrait painted by Sir Joshua Reynolds and died in England in 1798.

The crucial booklet is a small 50-page affair. On page 2,

Holwell says that "few survived capable of giving any detail of the manner in which it happened, and of these none but myself have attempted it." On page 9, he states that the Black Hole was a regular English prison in which persons were jailed; apparently in the mid-eighteenth century, before modern penal reform had started, all jails were literally dungeons admitting little light or air. The term appears to have been a popular local label much like "Black Maria" today. The symbolic connotation of black in reference to the alleged tragedy is not authentic or original, but a later attribute; most people still assume the name was bestowed after the event, which is not the case.

Holwell claims he was among the first to enter and "got possession of the window nearest the door." On page 10, he speaks of 146 "wretches" crammed together in a cube of about 18 ft." with two windows on the west wall opening on a veranda or piazza. There is no mention of any resistance or protest during the process of being herded in, a strange bit of submissiveness for doughty warriors to display in this extraordinary situation. Many were already wounded from the day's fighting (p. 12). He describes the sequence of perspiration, thirst, complete removal of clothing, and a vain attempt to bribe the guard with 2,000 rupees. Men who sat down and could not get up again "were instantly trod to death" by their fellows (p. 15). The guards allowed water to be brought; it was drunk in hats passed through the bars. But the rush for the water caused more trampling and pressing to death (p. 19). On page 23, Holwell declares that water gave no relief, but actually increased the thirst, so that the prisoners began to lick their own perspiration by preference. Even though ordinary water was apparently available in sufficient quantities, he and his associates preferred to drink the sweat wrung from each other's clothes. "No Bristol water can be more soft and pleasant than what arose from perspiration."

Then they began to insult the guards, hoping to be shot and thus put out of their misery. Holwell himself confesses he thought of opening his arteries and committing suicide. On page 29, he says, "Several of the inner ranks appeared to be dead

standing" — to which he adds an explanatory footnote properly asterisked, "Unable to fall by the throng and equal pressure round." This detail is quite a strain on one's credulity, as are a few of the other items here quoted, about which the reader is asked to reflect according to the principles of relational thinking.

About 2:30 a.m., Holwell swooned and knew no more until he was released near 6 in the morning on orders of the ruling prince who had heard of the havoc. "The rest, who survived the fatal night, gained their liberty, except Mrs. Carey (whose husband had perished), who was too young and handsome." This quaint observation adds piquancy to the atrocity tale. The rest of the account is unimportant, except that Holwell attributes his early release to the humane intervention of the dowager princess grandmother, who was stirred to pity by the sight of the prisoners marching through the regional capital in chains.

On the last page, Holwell gives a "List of the Smothered" (exclusive of 69 . . . Dutch, Portuguese, and colored natives) "whose names I am unacquainted with." The usual impression that only Englishmen were confined is not conveyed by the original, which plainly declares that about one-half were not Britishers.

This story is about as well-known in the English-speaking world as the fact that Napoleon was Emperor of France. Macaulay and Mill — the latter somewhat skeptically — lent the weight of their great names to its historicity. The building in which the incident is supposed to have occurred has long since disappeared, making way for a customs house erected before 1800. Disputes centering about the the actual size of the room are consequently futile, for no way exists of accurately determining the original dimensions.

Lord Curzon, while Viceroy, placed a memorial plaque near the spot in 1902, at which time Sir Rabindranath Tagore and other distinguished Bengalis publicly expressed their skepticism. Mussulman chronicles of the mid-eighteenth century period do not mention this spectacular event, even though they record and vigorously object to much smaller offenses committed by their own

countrymen. Neither official or unofficial Council, Company, or State records of the months immediately following the occasion mention the affair or any controversy specifically arising out of it.

Yet critical response to the legend is of remarkably recent date. In the *Calcutta University Magazine* for 1895, Dr. Bola Nath Chunder declared, "I have a very doubtful faith in this account." Akshay Kumar Maitra, author of *Shiraj-ud-daula* (a Bengali life of the native prince involved) considered the Black Hole tragedy a pure myth. In his scholarly *Rise of the Christian Power in India*, 1931, B. D. Basu observes, "Whether such a tragedy actually occurred is more than doubtful" (p. 57). Professor I. Gurumukh Singh of Benares remarks, "The majority of Indian writers and some of the English and foreign historians believe that the event never took place." [Pages 17-18 of his *Landmarks in Indian Constitutional and National Development*, Benares, 1933.] Dr. Taraknath Das, of the College of the City of New York, who has been consulted in the preparation of this report, shares the all but unanimous skepticism of native scholars on this point.

But the most important critical analysis appears in a brilliant monograph by J. H. Little entitled "The Black Hole — the Question of Holwell's Veracity", which appeared in the official Journal of the Calcutta Historical Society known as *Bengal, Past and Present*, 1915, Serial #21, pages 75-104. Little demonstrates that Holwell's credibility was not ranked high by his co-religionists and compatriots, who considered him a teller of specious fibs. Specifically, Little shows that Holwell (1) fabricated a speech and fathered it on the Nawab Alivardi Khan; (2) brought false charges against the British puppet ruler of Bengal, the Nawab Mir Jafar, accusing him of massacring persons all of whom were later shown to be alive — this libelous charge being repudiated and denounced by a Report to the London Directors from Fort William, dated Sept. 30, 1766; (3) forged a whole book and called it a translation from the ancient sacred writings of the Hindus. This last might be defended on Holwell's behalf if we assume him to have been victimized by some Brahmin or

pundit who enjoyed pulling a foreigner's leg; but certainly the first two cases have a brazen political significance also possessed by the similar story of the Black Hole.

Little examines the original "letter" line by line and comes to the conclusion that the whole thing is a gigantic hoax. The *internal* evidence (see some of the citations above) appears to support this verdict. Holwell says at the very start "few amongst us had the least idea of the dimensions or nature of the place we had never seen." This is strange when one recalls that he himself says that the Prince asked where the British customarily kept their own prisoners before ordering their confinement therein. Since Holwell was a magistrate in the colony (then quite small), he must have had some notion of what the local jail was like. Again, it was night-time; yet Holwell saw everything quite plainly, particularly the facial expressions of people at the far end of the room, — all in a room with two small windows opening on a covered porch. Holwell himself declares that when the guards outside wanted to see what was happening among the prisoners, they had to hold torchlights to the bars. If we review his document the way Shakespearean scholars dissect *Romeo and Juliet* to determine the approximate date of its composition, the probity of Holwell as an author becomes thoroughly suspect.

Strong *external* evidence against his story is that the subsequent Treaty of Alinagar, 1757, makes no mention of compensation to the widows and children of the men there murdered, although it stipulates reparation for a number of much less hideous war-time misdeeds. In a minute comparison of every single name mentioned in the London *Chronicle* (a contemporary newspaper) casualty list of those who perished in the actual fighting and defense of Fort William with those reported by Holwell as having been suffocated, Little reaches the conclusion that nine men only were in the Black Hole, including Holwell himself, and that two wounded soldiers probably did die there from their injuries, as well as an old clergyman who was sick before being captured. This seems to be the kernel of truth which was later distorted beyond recognition.

To repeat: The best interpretation one can make is that the names Holwell lists (a feature which lends maximum credibility to his tale) are those of persons who died in the assault on the Fort. Both Moslem and Hindu records agree with the English documents in the fierce resistance made by a small garrison of about 180-200 men; it is unlikely, therefore, that 146 were alive when captured. A London newspaper, the *Chronicle*, printed an obscure routine report listing almost all of the same men who Holwell said were with him in the Hole as having been killed in the storming of the Fort.

At a special meeting called for the purpose of settling the controversy provoked by the publication of Little's remarkable paper, the Calcutta Historical Society assembled on March 24, 1916, and reported the full Proceedings of this Debate in their journal, serial No. 23, pages 136-171. Various specialists analyzed the evidence fully for and against. The longest and best-informed discussions (notably one by Akshay Kumar Maitra) are anti-Holwell, and need not be further elaborated here.

A restrained and curiously brief defense of the traditional account is offered in the *Cambridge History of India*, Vol. 5, page 156, by H. H. Dodwell, a London authority on the history of the Asiatic dominions. Yet even he is forced to acknowledge that "Everyone who has studied the records of the time must have come to the conclusion that Holwell was not a virtuous man; it is even likely that he touched up his story so as to make the part he played as conspicuous as possible. . . . The truth is that we have not the material to decide what may have been the exact number of persons remaining after the capitulation."

What were Holwell's motives in inventing, or at least seriously embroidering, his personal experiences? We know he was politically ambitious and that his adroit identification of himself with an exciting affair (cf. his preface to the pamphlet) apparently helped win his appointment to the governorship. British public opinion might have supported this elevation as a form of poetic justice. Unlike Irving's legend of the Headless Horseman, the story is not intrinsically improbable, save that it implies gross cruelty which is inconsistent with letting Holwell go scot-free

almost immediately or even with permitting any of the captives to survive.

The reader is invited to form his own judgment about this familiar myth and to watch his own "will to believe" (or disbelieve) in active operation. Some questions plainly remain unanswered. Perhaps it is not excessive cynicism to prophesy that two centuries hence more people will continue to accept than to reject the dominant current account. Errors perpetuate themselves *via* closure and other mental forces with a vigor that remains a constant surprise to all but the most sophisticated devotees of truth.

PATTERNS OF AGGRESSIVE BEHAVIOR IN EXPERIMENTALLY CREATED "SOCIAL CLIMATES"

KURT LEWIN, RONALD LIPPITT, AND RALPH K. WHITE

Child Welfare Research Station, State University of Iowa

A. PROBLEMS AND METHODS

The present report is a preliminary summary on one phase of a series of experimental studies of group life which has as its aim a scientific approach to such questions as the following: What underlies such differing patterns of group behavior as rebellion against authority, persecution of a scapegoat, apathetic submissiveness to authoritarian domination, or attack upon an outgroup? How may differences in subgroup structure, group stratification, and potency of ego-centered and group-centered goals be utilized as criteria for predicting the social resultants of different group atmospheres? Is not democratic group life more pleasant, but authoritarianism more efficient? These are the sorts of questions to which "opinionated" answers are many and varied today, and to which scientific answers, are, on that account, all the more necessary. An experimental approach to the phenomena of group life obviously raises many difficulties of creation and scientific control, but the fruitfulness of the method seems to compensate for the added experimental problems.

In the first experiment Lippitt organized two clubs of 10-year-old children, who engaged in the activity of theatrical mask-making for a period of three months. The same adult leader, changing his philosophy of leadership, led one club in an authoritarian manner and the other club in accordance with democratic techniques, while detailed observations were made by four ob-

servers. This study, reported in detail elsewhere (6), suggested more hypotheses than answers and led to a second and more extensive series of experiments by White and Lippitt. Four new clubs of 10-year-old boys were organized, on a voluntary basis as before, the variety of club activities was extended, while four different adult leaders participated. To the variables of authoritarian and democratic procedure was added a third, "*laissez-faire*" or group life without adult participation. Also the behavior of each club was studied in different "social climates." Every six weeks each group had a new leader with a different technique of leadership, each club having three leaders during the course of the five months of the experiment series. The data on aggressive behavior summarized in this paper are drawn from both series of experiments.

Some of the techniques used for the equating of groups have been described previously (4), but will be summarized here with the improvements in method of the second experiment. Before the clubs were organized the schoolroom group as a whole was studied. Using the sociometric technique developed by Moreno (8) the interpersonal relations of the children, in terms of rejections, friendships, and leadership, were ascertained. Teacher ratings on relevant items of social behavior (*e.g.*, teasing, showing off, obedience, physical energy) were secured, and observations were made on the playground and in the schoolroom by the investigators. The school records supplied information on intellectual status, physical status, and socio-economic background. From the larger number of eager volunteers in each room it was then possible to select from each schoolroom two five-member clubs, which were carefully equated on patterns of interpersonal relationships, intellectual, physical, and socio-economic status, in addition to personality characteristics. The attempt was not to equate the boys within a particular club, but to ensure the same pattern in each group as a whole.

In spite of the methods described above to control by selection some of the more elusive social variables, it was essential to use a number of experimental controls which would help to make the results more clear-cut. First of all, to check on the

"individuality" of the club as a whole, each group was studied in different social atmospheres so that it could be compared with itself. A second question raised by the first experiment was that concerning the personality of the leader as a factor in the creating of social atmospheres. The second experiment, with four leaders, makes possible a comparison of the authoritarianism and democracy of four different leaders, and the "*laissez-faire*" method of two different leaders. In two cases it is also possible to com-

TABLE 1

Authoritarian	Democratic	<i>Laissez-faire</i>
1. All determination of policy by the leader.	1. All policies a matter of group discussion and decision, encouraged and assisted by the leader.	1. Complete freedom for group or individual decision, without any leader participation.
2. Techniques and activity steps dictated by the authority, one at a time, so that future steps were always uncertain to a large degree.	2. Activity perspective gained during first discussion period. General steps to group goal sketched, and where technical advice was needed the leader suggested two or three alternative procedures from which choice could be made.	2. Various materials supplied by the leader, who made it clear that he would supply information when asked. He took no other part in work discussions.
3. The leader usually dictated the particular work task and work companions of each member.	3. The members were free to work with whomever they chose, and the division of tasks was left up to the group.	3. Complete non-participation by leader.
4. The dominator was "personal" in his praise and criticism of the work of each member, but remained aloof from active group participation except when demonstrating. He was friendly or impersonal rather than openly hostile.	4. The leader was "objective" or "fact-minded" in his praise and criticism, and tried to be a regular group member in spirit without doing too much of the work.	4. Very infrequent comments on member activities unless questioned, and no attempt to participate or interfere with the course of events.

pare the same atmosphere, created by two different leaders with the same club.

One other type of control seemed very important, the nature of the club activity, and the physical setting. Using the same clubrooms (two clubs met at the same time in adjacent but distinctly separate areas of the same large room) seemed to answer the latter problem, but the question of activity was more complex. The following technique was developed: a list of activities which were of interest to all the children was assembled (*e.g.*, mask-making, mural painting, soap carving, model airplane construction, etc). Meeting first, in chronological time, the democratic groups used these possibilities as the basis for discussion and voted upon their club activity. The authoritarian leaders were then ready, as their clubs met, to launch the same activity without choice by the members. The "*laissez-faire*" groups were acquainted with the variety of materials which were available, but they were not otherwise influenced in their choice of activity; in their case, consequently, the activity factor could not be completely controlled.

The contrasting methods of the leaders in creating the three types of group atmosphere may be briefly summarized as in Table 1.

It should be clear that due to the voluntary nature of the group participation, and the coöperation of the parents and school systems, no radically autocratic methods (*e.g.*, use of threats, instilling fear, etc.) were used. Fairly congenial extra-club relationships were maintained with each member by the leader.

The kinds of data collected during the course of the experiments may be classed roughly as: (*a*) pre-club data, described above in relation to the problem of equating the groups; (*b*) observations of behavior in the experimental situation; and (*c*) extra-club information.

Observations of club behavior consisted of:

(*a*). A quantitative running account of the social interactions of the five children and the leader, in terms of symbols for directive, complaint, and objective (fact-minded) approaches and responses, including a category of purposeful refusal to respond to a social approach.

(b). A minute by minute group structure analysis giving a record of: activity subgroupings, the activity goal of each subgroup was initiated by the leader or spontaneously formed by the children, and ratings on degree of unity of each subgrouping.

(c). An interpretive running account of significant member actions, and changes in dynamics of the group as a whole.

(d). Continuous stenographic records of all conversation.

(e). An interpretive running account of inter-club relationships.

(f). An "impressionistic" write-up by the leader as to what he saw and felt from within the group atmosphere during each meeting.

(g). Comments by guest observers.

(h). Movie records of several segments of club life.

All of these observations (except *f*, *g*, and *h*) were synchronized at minute intervals so that side by side they furnish a rather complete cross sectional picture of the ongoing life of the group. The major purpose of this experiment in methodology of observation was to record as fully and with as much insight as possible the total behavior of the group, a distinct break away from the usual procedure of recording only certain pre-determined symptoms of behavior. The second aim was to ascertain whether data collected by this method could be fruitfully analyzed from both a sociological and psychological point of view (5).

Extra-club information is of the following types:

(a). Interviews with each child by a friendly "non-club" person during each transition period (from one kind of group atmosphere and leader to another) and at the end of the experiment, concerning such items as comparison of present club leader with previous ones, with the teacher, and with parents; opinions on club activities; how the club could be run better; who were the best and poorest club members; what an ideal club leader would be like, etc.

(b). Interviews with the parents by the investigators, concentrating on kinds of discipline used in the home, status of the child in the family group (relations with siblings, etc.), personality ratings on the same scale used by the teachers, discussion of child's attitude toward the club, school, and other group activities.

(c). Talks with the teachers concerning the transfer to the school-room, of behavior patterns acquired in the club.

(d). Administration of a Rorschach test to each club member.

(e). Conversations with the children during two summer hikes arranged after the experiment was over.

These data were gathered with a view to correlating the individual pattern of behavior in the club situation with the types of group membership which existed outside the experiment, and with the more or less stable individual personality structure. The individual differences in "social plasticity" seem to be rather striking.

Two other points of experimental technique seem of interest. The first concerns the introduction of observers into the club situation. In Lippitt's first experiment it was found that four observers grouped around a table in a physically separated part of the club room attracted virtually no attention if it was explained at the first meeting that "those are some people interested in learning how a mask-making club goes; they have plenty to do so they won't bother us and we won't bother them." In the second experiment the arrangement was even more advantageous and seemed to make for equally unself-conscious behavior on the part of the clubs. In this set-up the lighting arrangement was such that the observers were grouped behind a low burlap wall in a darkly shaded area, and seemed "not to exist at all" as far as the children and leaders were concerned.

The second point of interest is the development of a number of "group test" situations, which aided greatly in getting at the actual social dynamics of a given group atmosphere. One test used systematically was for the leader to leave the room on business during the course of the club meeting, so that the "social pressure" factor could be analyzed more realistically. Another practice was for the leader to arrive a few minutes late so that the observers could record the individual and "atmospheric" differences in spontaneous work initiation and work perspective. A third fruitful technique was that of having a stranger (a graduate student who played the rôle of a janitor or electrician) enter the club situation and criticize the group's work efforts. A rather dramatic picture of the results of this type of situation may be seen in Figures 5 and 6. Further variations of such experimental manipulations are being utilized in a research now in progress.

B. RESULTS

The analysis of the results from the second experiment is now proceeding in various directions, following two main trends: (a) interpretation of sociological or "group-centered" data; (b) interpretation of psychological or "individual-centered" data. The sociological approach includes such analyses as differences in volume of social interaction related to social atmosphere, nature of club activity, outgroup relationship, differences in pattern of interaction related to outgroup and ingroup orientation, atmosphere differences in leader-group relationship, effect upon group structure pattern of social atmosphere and types of activity, group differences in language behavior, etc. The psychological approach includes such analyses as relation of home background to pattern of club behavior, range of variation of member behavior in different types of social atmosphere, patterns of individual reaction to atmosphere transitions in relation to case history data, correlation between position in group stratification and pattern of social action, etc. In this paper will be presented only certain data from the partially completed general analysis which are relevant to the dynamics of individual and group aggression.

We might first recall one or two of the most striking results of the first experiment (6). As the club meetings progressed the authoritarian club members developed a pattern of aggressive domination toward one another, and their relation to the leader was one of submission or of persistent demands for attention. The interactions in the democratic club were more spontaneous, more fact-minded, and friendly. Relations to the leader were free and on an "equality basis." Comparing the two groups on the one item of overt hostility the authoritarian group was surprisingly more aggressive, the ratio being 40 to 1. Comparing a constellation of "ego-involved" types of language behavior (*e.g.*, hostile, resistant, demands for attention, hostile criticism, expression of competition) with a group of objective or "non-emotive" behaviors, it was found that in the authoritarian group 73 per cent of the analyzed language behavior was of the "ego-involved" type as compared to 31 per cent in the democratic club. Into the objective category went 69 per cent of the behavior of the demo-

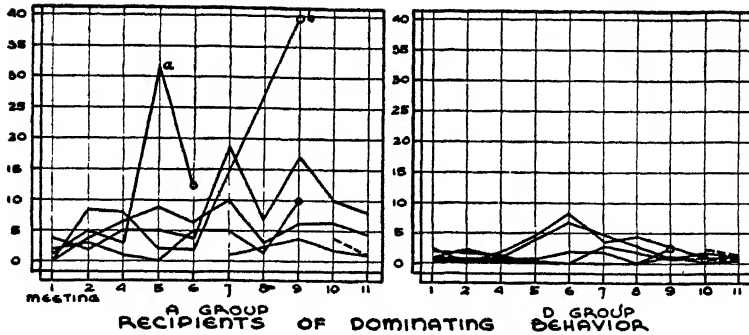


FIGURE 1

THE EMERGENCE OF SCAPEGOATS IN AN AUTOCRATIC ATMOSPHERE
(LIPPITT, 1937)

The curves (which indicate the amount of aggression directed against each individual) show a much lower general level of dominating behavior in the democratic (*D*) than in the autocratic (*A*) group. Twice during the meetings of the authoritarian club the aggression of four members was focused upon the fifth (*a* and *b*). In both cases the scapegoat dropped out of the group immediately or soon afterwards.

cratic group as compared to 37 per cent of the language activities of the authoritarian group.

A second type of data related to the dynamics of aggression as it existed in the first experiment may be seen in Figure 1. Twice during the course of the meetings of the authoritarian club the situation shifted from one of mutual aggression between all members to one of concentrated aggression toward one member by the other four. In both cases the lowered status of a scapegoat position was so acutely unpleasant that the member left the group, rationalizing his break from the club by such remarks as, "The doctor says my eyes are so bad I'll have to play outdoors in the sunshine instead of coming to club meetings." Interestingly enough, the two members who were singled out for persecution had been rated by the teachers as the two leaders in the group, one of them scoring second in popularity by the sociometric technique, as well as being physically the strongest. After the emergence of both scapegoats, there was a rather brief rise in friendly coöperative behavior between the other members of the group.

In the second experiment (see previous discussion, p. 4)

there were five democratic, five autocratic, and two "*laissez-faire*" atmospheres. The fact that the leaders were successful in modifying their behavior to correspond to these three philosophies of leadership is clear on the basis of several quantitative indices. For instance, the ratio of "directive" to "complaint" behavior on the part of the autocratic leaders was 63 to 1; on the part of the democratic leaders it was 1.1 to 1. The total amount of leader participation was less than half as great in "*laissez-faire*" as in either autocracy or democracy.

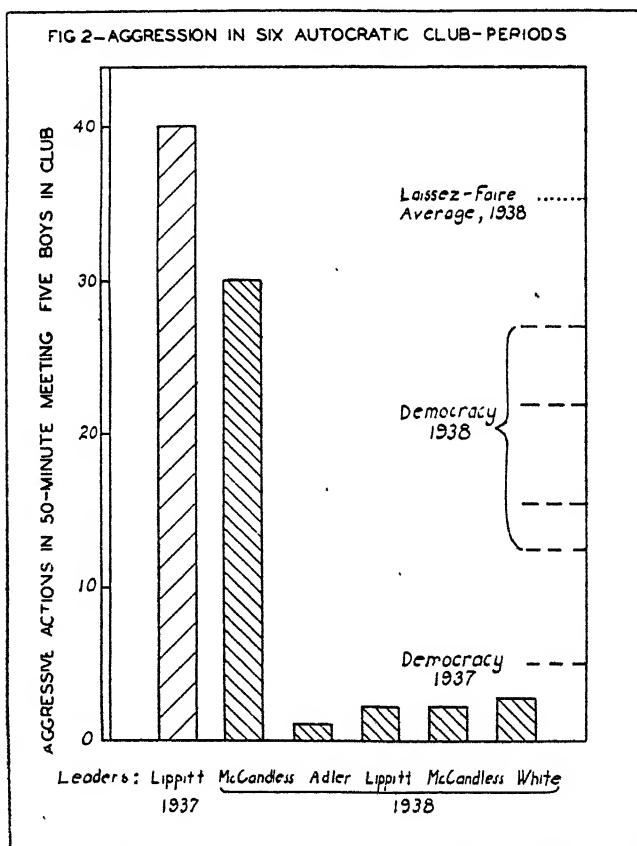


FIGURE 2

AGGRESSION IN AUTOCRACY

The amount of aggression is either very great or very small compared with aggression in democracy.

The data on aggression averages in these three atmospheres are summarized in Figures 2, 3, and 4. All of them indicate average amounts of aggression per 50-minute, five-member club meeting. They represent behavior records, as recorded by the interaction observer, and include all social actions, both verbal and physical, which he designated as "hostile" or "joking hostile." Figure 2 shows especially the bimodal character of the aggression averages in autocracy; four of the five autocracies had an extremely low level of aggression, and the fifth had an extremely high one. For comparison, a sixth bar has been added to represent aggression in Lippitt's 1937 experiment, computed on the same basis. It is obviously comparable with the single case of exceptionally aggressive behavior in the 1938 experiment. For comparison, also, four lines have been added which indicate the

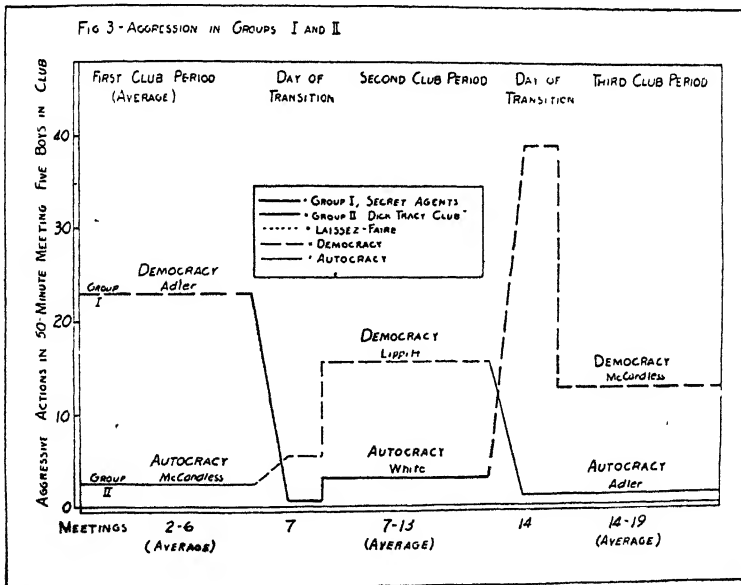


FIGURE 3

THE SAME GROUP IN DIFFERENT ATMOSPHERES

In each group, aggression was at a medium level in democracy and at a very low level in autocracy. Note that the leaders in the third period were the same as in the first, but reversed. Note also the sharp rise of aggression in one group on the day of transition to democracy. Group I shows "release of tension" on the first day of freedom (14) after apathetic autocracy. The name of the leader is indicated below that of the atmosphere.

aggression level in the two *laissez-faire* groups, in the four 1938 democracies, and in Lippitt's 1937 democracy. It can be seen that two of the six autocracies are above the entire range of democracies, and are in this respect comparable with the two *laissez-faire* groups. The other four autocracies are at the opposite extreme, below the entire range of the democracies.

Figures 3 and 4 show especially the character of the experimental controls. Together, they show how each of four groups was carried through three different periods with three different adult leaders. The relative importance of the deliberately created social atmosphere, as compared with either the personality make-up of the group or the personality of the adult leader, can be estimated from the character of these curves. It is clear that the same group usually changes markedly, and sometimes to an extreme degree, when it is changed to a new atmosphere under a different leader. In such transitions the factor of group personnel is held relatively constant, while the factors of leader personality and social atmosphere are varied. In addition, the factor of leader personality was systematically varied, as can be seen if the four curves are compared with one another. Each of the four leaders played the rôle of an autocrat at least once; two of them (Adler and White) played in addition the rôle of bystander in a "*laissez-faire*" group. One leader (Lippitt) was democratic with two different groups; and one (McCandless) was autocratic with two different groups. Through this systematic variation of both club personnel and leader's personality, the effects of the deliberately created social atmosphere (autocracy, democracy, *laissez-faire*) stand out more clearly and more reliably than would otherwise be possible.

In Figure 3, for instance, the two curves both tell the same story: a moderate amount of aggression in democracy and an abnormally small amount in autocracy, regardless of the personality of the leader (note that the rôles of Lippitt and McCandless were reversed, with each playing once the rôle of autocrat and once the rôle of democratic leader), and regardless of the personnel of the group itself (note that the curves cross when the atmospheres are reversed, and cross back again when the atmos-

phers return to what they were at the beginning). In Figure 4, the two *laissez-faire* atmospheres give very high levels of aggression although different groups and different leaders are involved. The most extreme change of behavior recorded in any group occurred when Group IV was changed from autocracy (in which it had shown the apathetic reaction) to *laissez-faire*. One of the autocratic groups (Figure 4) reacted apathetically, the other very aggressively. The aggressiveness of Group III may be due to the personalities of the boys, or to the fact that they had just previously "run wild" in *laissez-faire*.

The average number of aggressive actions per meeting in the different atmospheres was as follows:

<i>Laissez-faire</i>	38
Autocracy (aggressive reaction)	30
Democracy	20
Autocracy (apathetic reaction)	2

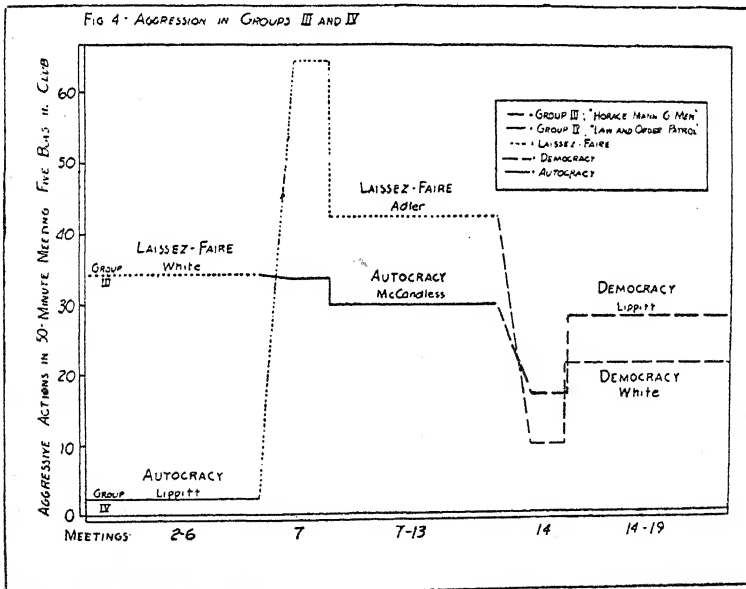


FIGURE 4

THE SAME GROUP IN DIFFERENT ATMOSPHERES

Group IV shows changes to the levels typical for each atmosphere. It shows also the "release of tension" on the first day of freedom (7) after apathetic autocracy. Group III seemed resistant to change; it was relatively aggressive even in democracy.

Critical ratios for these comparisons have not yet been computed. The data are comparable, however, with Lippitt's 1937 data, in which the critical ratios for the more important indices ranged between 4.5 and 7.5.

In the interpretation of these data it is natural to ask: Why are the results for autocracy paradoxical? Why is the reaction to autocracy sometimes very aggressive, with much rebellion or persecution of scapegoats, and sometimes very non-aggressive? Are the underlying dynamics in these two cases as different as the surface behavior? The high level of aggression in some autocracies has often been interpreted mainly in terms of tension, which presumably results from frustration of individual goals. Is it, then, an indication of non-frustration when the aggression level in some other autocracies is found to be extremely low?

Four lines of evidence in our experiments indicate that this is not the case, and that the low level of aggression in the apathetic autocracies is not due to lack of frustration.

First of all, there are the sudden outbursts of aggression which occurred on the days of transition from a repressed autocratic atmosphere to the much freer atmosphere of democracy or *laissez-faire*. Two of these are well illustrated in Figure 4. The boys behaved just as if they had previously been in a state of bottled-up tension, which could not show itself overtly as long as the repressive influence of the autocrat was felt, but which burst out unmistakably when that pressure was removed.

A second and very similar type of evidence can be obtained from the records on the days when the leader left the room for 10 or 15 minutes. In the three other atmospheres (*laissez-faire*, aggressive autocracy, and democracy) the aggression level did not rise when the leader left the room. In the apathetic autocracies, however, the level of aggression rises very rapidly to 10 times its former level. These data should not be overstressed, because aggression even then does not rise to a level significantly above that of the other atmospheres. It is so extremely low in the apathetic atmosphere that even multiplication by 10 does not produce what could be called a high level of aggression. (The effect of the leader's absence is shown more significantly in a deteriora-

tion of work than in an outburst of aggression.) Nevertheless, the rapid disappearance of apathy when the leader goes out shows clearly that it was due to the repressive influence of the leader rather than to any particular absence of frustration. In this connection it should be added that the autocratic leader never forbade aggression. His "repressive influence" was not a prohibition created by explicit command but a sort of generalized inhibition or restraining force.

In the third place, there are the judgments of observers who found themselves using such terms as "dull," "lifeless," "submissive," repressed," and "apathetic" in describing the nonaggressive reaction to autocracy. There was little smiling, joking, freedom of movement, freedom of initiating new projects, etc.; talk was largely confined to the immediate activity in progress, and bodily tension was often manifested. Moving pictures tell the same story. The impression created was not one of acute discontent, by any means, and the activities themselves were apparently enjoyable enough so that the net result for most of the boys was more pleasant than unpleasant. Nevertheless, they could not be described as genuinely contented.

The fourth and perhaps the most convincing indication of the existence of frustration in these atmospheres is the testimony of the boys themselves. They were individually interviewed, just before each day of transition to a new atmosphere, and again at the end of the whole experiment. The interviewing was done by an adult who had not served as a leader in the boy's own group. On the whole good rapport was achieved, and the boys talked rather freely, comparing the three leaders under whom their club had been conducted. (For them it was a question of comparing leaders they liked or did not like, as they were unaware of the deliberate change in the behavior of the same leader from one atmosphere to another or of the nature of the experiment.) With surprising unanimity the boys agreed in a relative dislike for their autocratic leader regardless of his individual personality. Nineteen of the 20 boys liked their leader in democracy better than their leader in autocracy. The twentieth boy, as it happened, was the son of an army officer (the only one in the

group), and consciously put a high value upon strict discipline. As he expressed it, the autocrat leader *"was the strictest, and I like that a lot."* For the other 19, strictness was not necessarily a virtue, their description of the autocrat being that he was *"too strict."* Typical comments about the autocrat were: *"he didn't let us do what we wanted to do"; "he wouldn't let us go behind the burlap"; "he was all right mostly — sort of dictator-like"; "we just had to do things; he wanted us to get it done in a hurry"; "he made us make masks, and the boys didn't like that"; "the other two guys suggested and we could do it or not, but not with him"; "we didn't have any fun with him — we didn't have any fights."* Typical comments about the democratic leader were: *"he was a good sport, worked along with us and thinks of things just like we do"; "he never did try to be the boss, but we always had plenty to do"; "just the right combination — nothing I didn't like about him"; "we all liked him; he let us tear down the burlap and everything."* These comments were almost uniformly dependent upon the rôle played by the leader, and were exactly reversed when he played a different rôle.

As between the leaders in autocracy and *"laissez-faire,"* the preference was for the *"laissez-faire"* leader in seven cases out of ten. The three boys who preferred the autocrat made such comments about the *"laissez-faire"* leader as: *"he was too easy-going"; "he had too few things for us to do"; "he let us figure things out too much";* in contrast the autocrat *"told us what to do, and we had something to do all the time."* For the other seven, even disorder was preferable to rigidity: *"we could do what we pleased with him"; "he wasn't strict at all."*

Another form of aggression was outgroup hostility, as manifested especially in two "wars" between clubs meeting in the same large room at the same time. Both wars seemed to be mainly in a spirit of play. They were much more like snowball fights than serious conflicts. (This is one more reason why in this case one should be cautious in comparing adult political phenomena directly with our data on small groups of children.) Our two small "wars" are interesting in their own right, however,

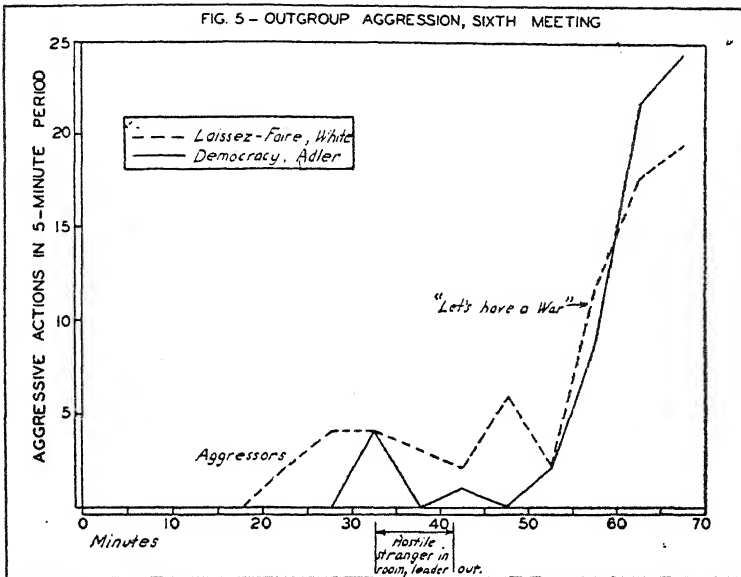


FIGURE 5

CONFLICT BETWEEN GROUPS AFTER INTRUSION OF HOSTILE STRANGER

After the stranger left, strong hostility developed between the two groups. Before the major conflict, minor hostilities had already occurred, with one or two members of the *laissez-faire* group playing the rôle of aggressors.

especially since the same general constellation of factors seemed to be operating in both cases.

The curves of rising hostility, computed for five-minute intervals, are shown in Figures 5 and 6. From these curves it can be seen that the first "war" started gradually, with a long period of minor bickering and name calling, followed by a much steeper gradient of increasing hostility. The overt hostilities consisted of throwing water, small pieces of clay (which nearly always missed their mark), and sometimes water color paint, flicked from the end of a long paint brush. No one was hurt. The second conflict (Figure 6) began much more suddenly. Name calling began in the first minute after the "hostile stranger" left the room, and almost immediately the boys seemed to remember their previous conflict and to wish a repetition of it. Beginning with verbal aggression such as, "Why don't you

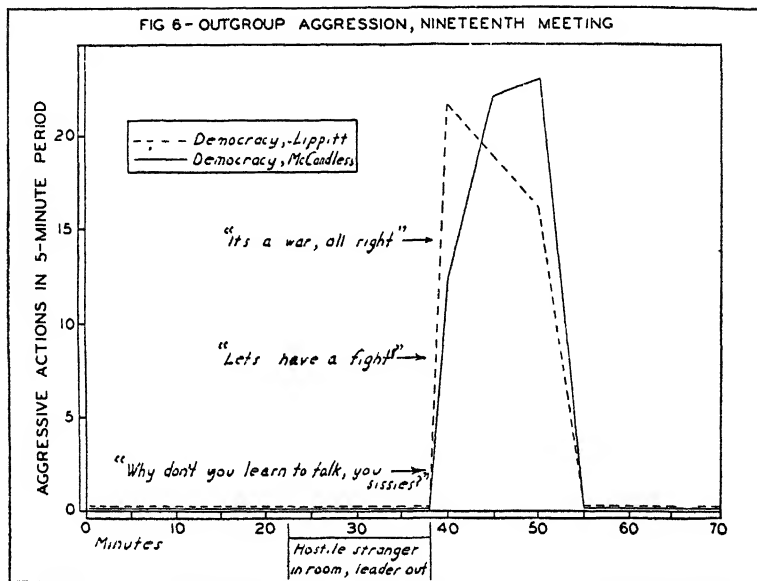


FIGURE 6

CONFLICT BETWEEN GROUPS AFTER INTRUSION OF HOSTILE STRANGER

The intrusion of a hostile stranger was followed by intergroup conflict (as in Figure 5). In this case the hostilities began suddenly, rising within four minutes almost to their maximum level.

learn to talk, you sissies?" they passed within three minutes to throwing small pieces of soap (small pieces of soap statuettes, which they had carved, were lying about), and within five minutes nearly all the boys on both sides were wholeheartedly participating. This difference in steepness of the hostility gradient was perhaps due in part to a higher level of tension or to weaker restraining forces on the later occasion, but it seemed to be due also to a cognitive difference. On the other occasion the pattern of intergroup conflict had been established; it was, by that time, a part of the boys' "cognitive structure" — a clearly defined region which they could enter or not as they chose; and since they had found the first "war" to be very pleasantly exciting, they readily and quickly entered the same region again when the general psychological situation was conducive to conflict. In this connection it may be noted that the second conflict was labelled

verbally almost immediately, while the first one was not labelled until it was already well under way. On the first occasion the shout, "Let's have a war!" went up long after the minor hostilities had begun; on the second occasion, one boy shouted, "Let's have a fight," only two minutes after the name calling began, and another one legalized it two minutes later with the words, "It's a war all right."

Certain similarities between the two days of conflict suggest some very tentative hypotheses as to the psychological factors conducive to this sort of conflict. In the first place, both occurred on days when, with the adult leader absent, a hostile stranger had been in the room and had criticized the work which the boys were doing. This had been deliberately planned as a "test situation"; a graduate student, playing the rôle of a janitor or an electrician, was the hostile stranger. It may be doubtful whether or not the term "substitute hate object" is an appropriate one here; but there was no question in the observers' minds that in both cases the intrusion of the stranger tended to disorganize the regular play activities of the clubs and to build up a tense, restless psychological condition which was conducive to inter-group conflict. In the second place, both conflicts started when no respected adult was present. In the first one the main aggressors were unquestionably the *laissez-faire* group (see Figure 5). Their leader was physically present at the time, but he was psychologically unimportant. The second conflict began when the leaders on both sides were out of the room, and by the time the leaders returned, it had gathered great momentum. In the third place, both conflicts occurred at a time when there was no absorbing group activity as an alternative. The first one began at a time when the members of the *laissez-faire* group seemed unusually bored and dissatisfied with their own lack of solid accomplishment. The second one began after the boys had become somewhat bored with their soap carving, and after this individualistic activity had been further disrupted by the criticisms of the stranger.

The free direct expression of aggression by the "wars" following frustration in the *laissez-faire* and democratic situations

offers a contrast to several other patterns of expression which were observed in some of the authoritarian situations. These types of behavior might be briefly labelled: (*a*) a "strike"; (*b*) rebellious attack; (*c*) reciprocal aggression among all members; (*d*) scapegoat attack; (*e*) release behavior after a decrease in leader pressure; (*f*) aggression against impersonal "substitute hate objects."

Both the "strike" and symptoms of rebellious action occurred in the aggressive type of autocracy. About the middle of the series of six meetings the club members went to their teacher with a letter of resignation signed by four of them. They asked their teacher to give this to the leader when he came to get them after school. The teacher refused to act as a go-between, suggesting that the boys go to the leader directly, but when he appeared after school, courage seemed to wane and they all went to the meeting as usual. Overt rebellious acts were of the following nature: breaking a rule by carving on the posts in the clubroom (while casting sidelong glances at the leader), deliberately walking behind the burlap walls of the clubroom without permission (mentioned to an interviewer), leaving the club meeting early, and pretending not to hear when spoken to by the leader. The third and fourth kinds of behavior were also typical of aggressive authoritarianism and have been mentioned in describing the first experiment during which two scapegoats emerged. As has been mentioned, changes in amount of aggression while the leader was out, and days of transition to a freer atmosphere were especially good indicators of the existence of unexpressed tension in the apathetic autocracies.

Two very interesting examples of what we have tentatively called "release behavior through an impersonal substitute hate object" are worthy of description. During the eleventh meeting of the first experiment the authoritarian group was given a chance to indicate by secret ballot whether they would like the club to stop or continue for several more meetings. We may go to an observer's record for further comments:

Peculiar actions follow the leader's announcement that because of the vote there will be no more meetings after today. The leader asks

RO and *J* to put the paper on the floor as usual. They put it down and then run and jump on it time and again in a wild manner. The group masks are divided among the members and *J* immediately begins to throw his around violently, pretending to jump on it. He throws it down again and again, laughing. *R* wants to know if it won't break, then starts to throw his down too. Later *J* and *RO* chase each other around the room wildly with streamers of towelling. . . .

Rather clearly the work products of this authoritarian atmosphere seemed to be the objects of aggressive attack rather than prideful ownership.

During a last meeting of the second experiment a rather similar burst of behavior occurred in one of the democratic groups. The group was highly involved in an activity of making an oil painting on glass. While the leader was out for a short time (by arrangement) a student in the janitor rôle came in to sweep. From the running accountant's record of the twenty-second minute we find,

He is making dirt fly and sweeping it toward the group. They all begin to cough but don't move from their work.

Several minutes later we find the comment,

Janitor has almost swept them away, but still no hostile response. The project seems to have a very high valence.

Five minutes later the janitor had gotten them out of their chairs in order to sweep, then

the janitor accidentally knocks a piece of their glass on the floor. They all yell and *R* makes as if to throw something at him. *F* says that if the leader were here he would beat up the janitor.

Five minutes later, after a number of comments criticizing the art work of the club, the janitor left. The members dropped their work completely, climbed the rafters and made considerable noise. On the thirty-sixth minute we find,

R comes down from the rafter and begins to complain about the janitor, *L* joins him and they all complain bitterly and loudly.

Within three minutes the group began to destroy a large wooden sign upon which they had painted the club name. Such comments as this appear in the running account,

F is wielding two hammers at once. . . . *R* is busy pulling out all the nails. . . . They are excited. . . . *F* knocks the first hole through it. . . . *R* tries to caution *F* for a minute, and then gets busy himself . . . their unexpressed aggression toward the janitor is taking a violent

outlet . . . they are all very serious and vicious about the destruction of the sign . . . they seem to be getting a great deal of "pure animal pleasure" of the pillage.

The meeting ended with three or four minutes of pleasant conversation.

C. INTERPRETIVE COMMENTS

From the many theoretical problems involved we should like to discuss but one, namely, the problem of aggression and apathy. Even here we wish to show the complexity of the problem and its possible attack from a field theoretical point of view rather than to set forth a definite theory.

It is not easy to say what aggression is, that is, if one is not satisfied with mere verbal definition. One important aspect obviously is that one group or an individual within a group turns against another group (or individual). In case these groups are subgroups of one original group, it can be called aggression *within a group*, otherwise aggression *against an outgroup*.

Both kinds of aggression occurred in our experiments. All of these aggressions were spontaneous in character. In other words, it was not a situation where a group of people are ordered by a politically dominating power (like the state) to indulge in a certain type of directed activity called war. On the whole the aggression was the outcome of the momentary emotional situation, although in two cases the aggressions had definitely the character of a fight of one group against another group and showed a certain amount of coöperative organization within each group.

It is necessary to mention four points which seems to play a dominant rôle in the spontaneous aggressions: tension, the space of free movement, rigidity of group structure, and the style of living (culture).

1. *Tension*

An instance where tension was created by *annoying* experiences occurred when the group work was criticized by a stranger (janitor). There were two cases where fighting broke out immediately afterwards.

In the autocratic atmosphere the behavior of the leader probably annoyed the children considerably (to judge from the interviews reported above).

In addition, there were six times as many directing approaches to an individual by the leader in autocracy than in democracy (Figure 7). It is probably fair to assume that the

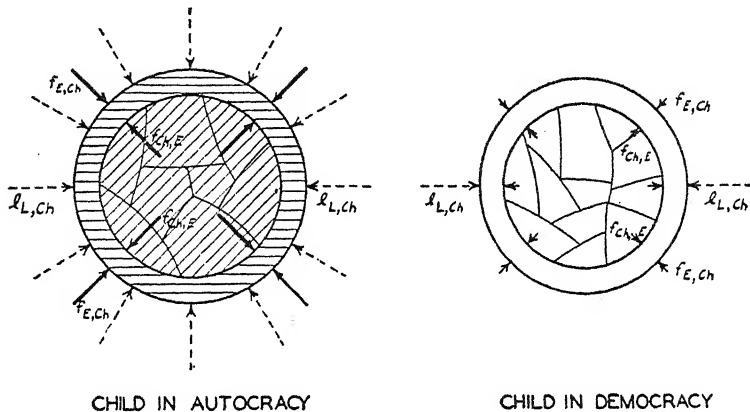


FIGURE 7
LEADER PRESSURE AND CHILD TENSION

In the authoritarian situation the leader makes six times as many directing approaches ($f_{L,Ch}$) to the child member as in the democratic situation. This creates social pressure (equivalent to forces $f_{E,Ch}$ of the environment on the child) and therefore a higher state of tension in the child in the autocratic group; this tension demands some sort of outlet toward the environment (equivalent to forces $f_{Ch,E}$).

bombardment with such frequent ascendant approaches is equivalent to higher pressure and that this pressure created a higher tension.

2. Narrow Space of Free Movement as a Source of Tension.

On the whole, even more important than this single annoying experience was the general atmosphere of the situation. Experiments in individual psychology (1) seemed to indicate that lack of space of free movement is equivalent to higher pressure; both conditions seem to create tension. This seemed particularly true if an originally larger space was narrowed down (one is reminded here of the physical tension created by decreasing volume, although one should not overstress the analogy).

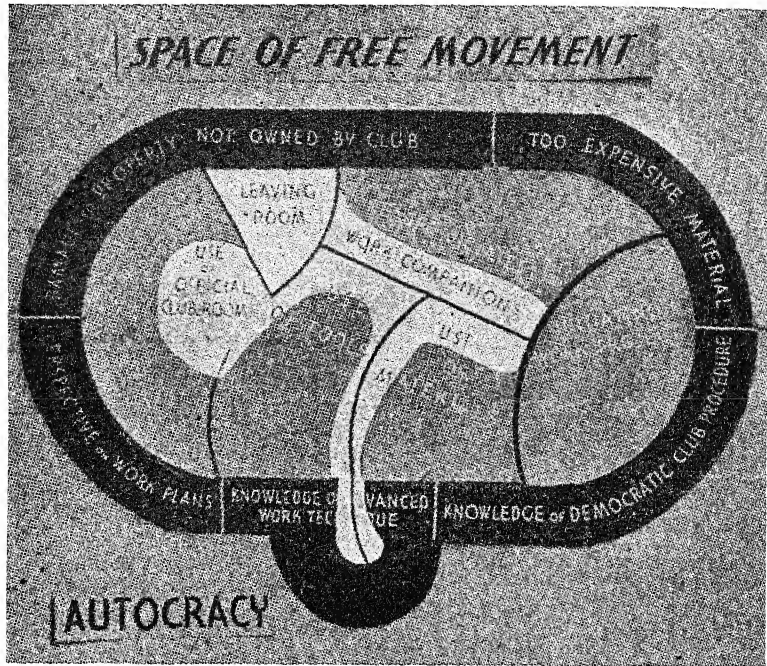
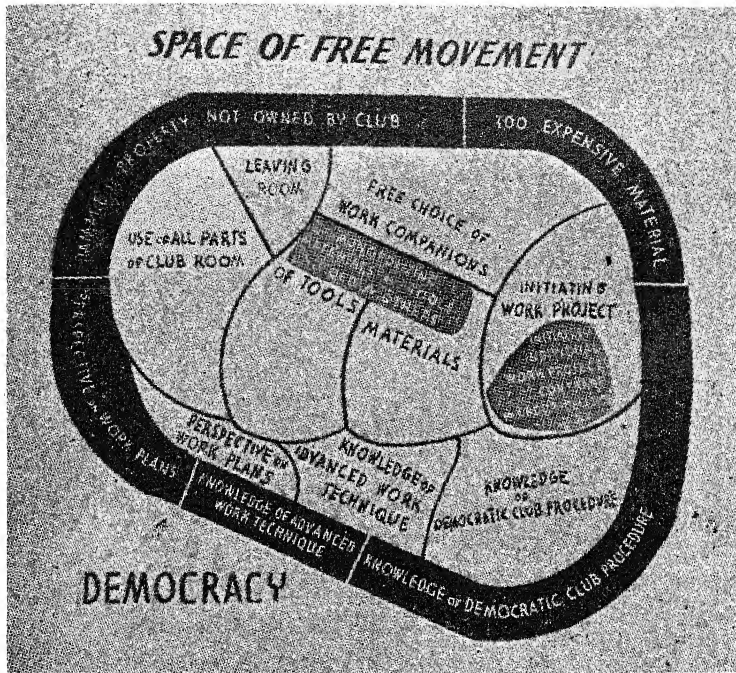


FIGURE 8 (See Opposite Page)

Our experiments seemed to indicate that a similar relation between the narrow space of free movement and high tension holds also in regard to groups. The space of free movement in autocracy was smaller in relation to the activities permitted and the social status which could be reached (Figures 8 and 9). In *Laissez-faire*, contrary to expectations, the space of free movement was not larger but smaller than in democracy, partly because of the lack of time perspective and partly because of the interference of the work of one individual with the activities of his fellows.

3. Aggression as the Effect of Tension

The annoying occurrences, the pressure applied by the leader, and the lack of space of free movement, are three basic facts which brought up a higher tension. Our experiments indicate that this higher tension might suffice to create aggression. This



FIGURES 8 AND 9

SPACE OF FREE MOVEMENT IN AUTOCRACY AND DEMOCRACY

In the autocratic situation the space of free movement (white) was originally bounded only by the limitation in ability and knowledge (black) of the members, but was soon limited much further by the social influence of the leader (gray).

In democracy the space was increased with the help of the leader.

seems to be of theoretical importance; obviously some aggressive acts can be viewed mainly as a kind of "purposive" action (for instance, to destroy a danger), and one might ask whether or not this component is an essential part in the causation of any aggression. In our experiments, the two wars between the two outgroups can hardly be classified in this way. They seemed to be rather clear cases where aggression was "emotional expression" of an underlying tension.

4. Rigidity of Group Structure

However, to understand aggression one will have to realize that tension is only one of the factors which determine whether

or not an aggressive action will take place. The building up of tension can be said to be equivalent to the creation of a certain type of need which might express itself in aggressive action. Tension sets up the driving force (2) for the aggression (in the two situations with which we are dealing). However, whether these driving forces actually lead to aggression or to some other behavior, for instance that of leaving the group, depends on additional characteristics of the situation as a whole. One of these seems to be the rigidity of the social position of the person within the group.

Aggression within a group can be viewed as a process by which one part of the group sets itself in opposition to another part of the group, in this way breaking the unity of the group. Of course, this separation is only of a certain degree.

In other words, if M indicates a member or subgroup and Gr the whole group, an aggression involves a force acting on the

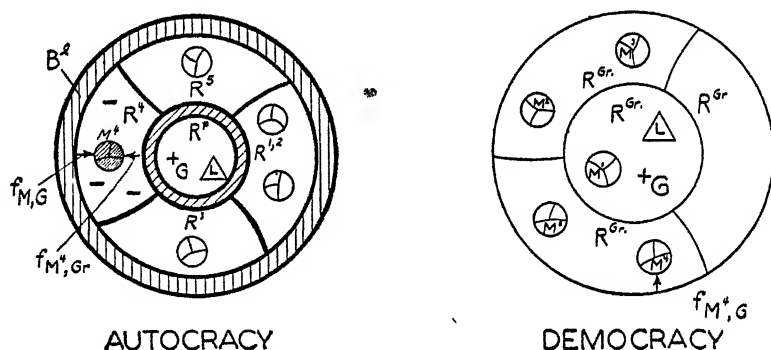


FIGURE 10

RIGIDITY OF GROUP STRUCTURE A. A TENSION FACTOR

In autocracy where each member or subgroup ($M^1, M^2 \dots M^5$) has a circumscribed region of activity ($R^1, R^2 \dots R^5$), and especially where the central regions of group life (policy formation R^p), are inaccessible to most members, rigid barriers (B) to own goals (G) continually frustrate members' efforts. The member's own position in the group structure (R^i) therefore acquires a negative valence, usually creating a force away from group membership ($f_{M^i, -Gr}$). But in rigid group structures a restraining barrier (B) keeps members or subgroups from leaving until a very high state of tension develops.

In democracy where all group regions (R^{Gr}) are accessible to all members ($M^1, M^2 \dots M^5$), their own goals (G) are more easily attained and no such frustrating situation develops.

subgroup in the direction away from the main group ($f M, -Gr$) or other part of the subgroup. From this it should follow theoretically that if a subgroup can easily locomote in the direction away from the group it will do so in case this force shows any significant strength. In other words, a strong tension and an actual aggression will be built up only in case there exist forces which hinder the subgroup from leaving the group (Figure 10).

Cultural anthropology gives examples which might be interpreted from this angle. The Arapesh (7), for instance, are living in a society where everyone is a member of a great variety of different groups and seems to shift easily from one group to another; it is a society without rigidly fixed social position. The fact that they show extremely little aggression might well be linked with this lack of rigid social structure.

Another example might be seen in the fact that adolescents who have been kept within the family probably show more aggression. In other words, the more rigid the family structure the more difficult it is for them to move from childhood to adulthood.

An additional example is the well-known fact that narrow family ties which serve to make it difficult for husband and wife to leave each other may make aggression between them particularly violent.

In our experiment, autocracy provided a much more rigid social group than democracy. It was particularly difficult for the members of an autocracy to change their social status (3). On the other hand, in both groups the members did not like to leave the group as a whole because of the interest in the work project and the feeling of responsibility to the adult leader.

On the whole, then, the rigidity of the group will function as a restraining force (2) against locomotion away from the group, or from the position within the group. Sufficient strength of this restraining force seems to be one of the conditions for the building up of a tension which is sufficiently high to lead to aggression.

It can be seen easily that the barriers limiting the space of free movement may have a similar function. We mentioned

above, that a narrow space of free movement seems to be equivalent to pressure, and, in this way, creates tension. At the same time, the barriers prevent locomotion, thus providing the restraining forces necessary for building up higher tension.

It was already mentioned that these restraining forces are particularly strong in our autocratic group (Figure 10).

5. *Style of Living (Culture)*

Whether or not a given amount of tension and given restraining forces will cause a person to become aggressive depends finally upon the particular patterns of action which are customarily used in the culture in which he lives. The different styles of living can be viewed as different ways a given problem is usually solved. A person living in a culture where a show of dominance is "the thing to do" under certain conditions will hardly think of any other way in which the solution of this problem may be approached. Such social patterns are comparable to "habits." Indeed, individual habits as well as cultural patterns have dynamically the character of restraining forces against leaving the

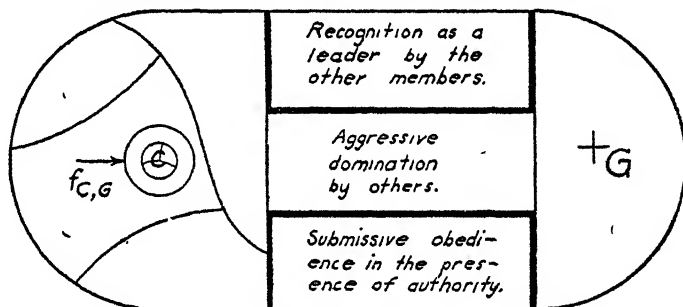


FIGURE 11

DIFFERENT STYLE OF LIVING A. REPRESENTED BY DIFFERENT DISTINGUISHED PATHS (AGGRESSIVE AUTOCRACY)

The goal (G) of maximum social status and space of free movement can be reached by one or more of several procedures depending on actual possibilities and the prevailing mode of behavior in that group. In our "experimentally created cultures," the distinguished path to G was for a child (C) in aggressive autocracy that of aggressive domination of other members. In a similar situation the distinguished path for a member of democratic groups seemed to be that of gaining voluntary recognition of the other members as a leader through work and social efforts. In the situation of apathetic authoritarianism the path seemed to be that of submissive obedience to authority, which might win praise from the leader.

paths determined by these patterns. In addition, they determine the cognitive structure which a given situation is likely to have for a given individual.

For the problem of aggression, this cultural pattern, determined by the group in which an individual lives and by his past history, is of great importance. It determines under what conditions aggression will be, for the individual concerned, the "distinguished path" to the goal (2). It determines, furthermore, how easily a situation will show for him a cognitive structure where aggression appears to be one possible path for his action (Figure 11).

The factors named are sufficient to warn against any "one-factor" theory of aggression. Here, as in regard to any other behavior, it is the specific constellation of the field as a whole that determines whether or not aggression will occur. In every case one has to consider both the driving and the restraining forces and the cognitive structure of the field. Such a field theoretical approach seems to be rather arduous. On the other hand, only in this way will one be able to understand for instance the paradox of behavior that autocracy may lead either to aggression or to apathy. It was stated that aggression is partly to be viewed as an emotional outbreak due to tension and that this tension, in turn, is due to pressure and restraining forces (lack of space of free movement). We have apathy when the pressure and the restraining forces from without are kept stronger than the forces ($f_{CH,E}$ in Figure 7) within the person which lead to the emotional expression, and are due to the tension. Whether or not the forces from without or those from within are stronger depends upon the absolute amount of pressure and also on the "willingness" of the person to "accept" the pressure.

The field theoretical approach also provides indications for the circumstances under which one might generalize the results of such experimental group studies. One must be careful of making too hasty generalization, perhaps especially in the field of political science. The varieties of democracies, autocracies, or "*laissez-faire*" atmospheres are, of course, very numerous. Besides, there are always individual differences of character and

background to consider. On the other hand, it would be wrong to minimize the possibility of generalization. The answer in social psychology and sociology has to be the same as in an experiment in any science. The essence of an experiment is to create a situation which shows a certain pattern. What happens depends by and large upon this pattern and is largely although not completely independent of the absolute size of the field. This is one of the reasons why experiments are possible and worthwhile.

The generalization from an experimental situation should, therefore, go always to those life situations which show the same or sufficiently similar general patterns. This statement includes both the rights and the limitations of generalization.

D. SUMMARY

1. In a first experiment, Lippitt compared one group of five 10-year-old children, under autocratic leadership, with a comparable group under democratic leadership. In a second experiment, Lippitt and White studied four comparable clubs of 10-year-old boys, each of which passed successively through three club periods in such a way that there were altogether five democratic periods, five autocratic periods, and two "*laissez-faire*" periods.

2. In the second experiment, the factor of personality differences in the boys was controlled by having each group pass through autocracy and then democracy, or vice versa. The factor of leader's personality was controlled by having each of four leaders play the rôle of autocrat and the role of democratic leader at least once.

3. Records on each club meeting include stenographic records of conversation, quantitative symbolic records of group structure, quantitative symbolic records of all social interactions, and a continuous interpretive running account. Parents and teachers were interviewed; each boy was given the Rorschach ink blots, a Moreno-type questionnaire, and was interviewed three times. Analysis of causal relationships between these various types of data is still far from complete. As a preliminary report we are giving here a part of the data bearing upon one specific problem, that of aggression.

4. In the first experiment, hostility was 30 times as frequent in the autocratic as in the democratic group. Aggression (including both "hostility" and "joking hostility") was 8 times as frequent. Much of this aggression was directed toward two successive scapegoats within the group; none of it was directed toward the autocrat.

5. In the second experiment, one of the five autocracies showed the same aggressive reaction as was found in the first experiment. In the other four autocracies, the boys showed an extremely nonaggressive, "apathetic" pattern of behavior.

6. Four types of evidence indicate that this lack of aggression was probably not caused by lack of frustration, but by the repressive influence of the autocrat: (a) outbursts of aggression on the days of transition to a freer atmosphere; (b) a sharp rise of aggression when the autocrat left the room; (c) other indications of generalized apathy, such as an absence of smiling and joking; and (d) the fact that 19 out of 20 boys liked their democratic leader better than their autocratic leader, and 7 out of 10 also liked their "*laissez-faire*" leader better.

7. There were two "wars," more or less playful, and without bodily damage, between clubs meeting in the same room at the same time. The first of these began gradually, the second suddenly. Three factors, present in both cases, seemed conducive to group conflict: (a) irritation and tension produced by a hostile stranger, (b) absence of a respected adult, and (c) lack of any absorbing alternative activity.

8. There were two striking instances of aggression against impersonal objects.

9. A general interpretation of the above data on aggression can be made in terms of four underlying factors: tension, restricted space of free movement, rigidity of group structure, and style of living (culture).

REFERENCES

1. LEWIN, K. A Dynamic Theory of Personality: Selected papers. Trans. by Donald K. Adams and Karl E. Zener. New York: McGraw-Hill, 1935. Pp ix+286.
2. ———. The conceptual representation and the measurement of psychological forces. *Duke Univ. Contrib. Psychol. The.*, 1938, 1, No. 4. Pp. 247.

3. ———. Experiments in social space. *Harvard Educ. Rev.*, 1939, 9, No. 1.
4. LEWIN, K., & LIPPITT, R. An experimental approach to the study of autocracy and democracy: A preliminary note. *Sociometry*, 1938, 1, 292-300.
5. ———. Field theory and experiment in social psychology. (Accepted for publication in the May issue of the *American Journal of Sociology*.)
6. LIPPITT, R. An experimental study of authoritarian and democratic group atmospheres. *Univ. Iowa Stud., Stud. Child Wel.*, 1939, 16, No. 3. [In press.]
7. MEAD, MARGARET [ED.]. *Cooperation and Competition among Primitive Peoples*. New York: Macmillan, 1937. Pp. viii+325.
8. MORENO, J. L. *Who Shall Survive? A new approach to the problem of human interrelations*. (Nervous and Mental Disease Monograph Series No. 58.) Washington, D. C.: Nervous & Mental Disease Publishing Co., 1934. Pp. xvi+437.

THE AUTHORITARIAN CHARACTER STRUCTURE *

A. H. MASLOW

Brooklyn College

In this war it is difficult to differentiate our friends from our enemies. The usual criteria that have been used in the past fail us now. But even so, our press and our leaders come back to them again for lack of something better. A fascist cannot be defined by his geographical location, his nominal national citizenship, the language he speaks, his religion, his skin color or other racial characteristics, his economic class, or even social caste. Any of these determiners may be involved in any individual case, but none of them will serve for all cases. To make the situation worse, we cannot even trust what people say or do, for personal expedience as well as covert loyalties may cause the most astounding shifts in policy or in behavior. It may be pointed out finally that in the last analysis even the conscious belief of the subjects about himself is not altogether trustworthy, for there are many who tend unconsciously in the authoritarian direction.

The psychologist now has available data and principles that can certainly help in clearing up these confusions, so that by

* This paper is the result of five years of off-and-on clinical study of authoritarian individuals in our society. This study was stimulated primarily by a series of lectures on the subject by Dr. Erich Fromm, then of the International Institute of Social Research. Fromm's recent book, *Escape from Freedom*, presents some of his conclusions on authoritarianism. Since I have found myself in disagreement with him at certain basic theoretical points (even though in agreement on most else) and since the subject is of such obvious importance today, it seemed justifiable to present this differently centered point of view of the same subject matter, even though he has discussed it so well. A summer's field work with the Northern Blackfoot Indians, made possible by a grant-in-aid from the Social Science Research Council, has also influenced this paper.

making basic issues more clear, he can help in the task of separating our friends from our enemies in our own country as well as others. These criteria are not offered as sufficient in themselves. That would be over-psychologizing of the worst sort. But, taken together with other characteristics of the individuals under consideration it should be possible with them to make diagnosis and understanding easier and more certain.

Any discussion of the concept of character structure must make its peace with field theory before it starts. It is common for any discussion of character structure to be attacked immediately on the grounds that it over-psychologizes what is essentially a person-world inter-relationship. I do not think this is true. It is certainly both useful and possible to focus our attention on one or the other member of this relationship. This paper proceeds from the conviction that the almost exclusive attention to economic, political, social, and other cultural forces to the neglect of the psychological factors involved, has been a dis-service to the true understanding of the inter-relationship between the individual and the world in which he lives.

The concept of character structure has, in any case, to be understood as a final crystallization of many determining forces. Of all these forces, it is conceded that the most important is probably all the situations or fields through which the organism has passed in its life history. That is, the character structure may be considered to be largely (though not altogether) the reflection in the individual of all the environmental forces that have ever impinged upon him. This is even more specifically true of the concept of the "world view" to be discussed below. I, therefore, conceive the concept of character structure, as well as world view, to be an intersection of psychological and sociological concepts.

THE WORLD VIEW (*Weltanschauung*)

Many characteristics of the authoritarian person are already well known. Some of them are listed below. But these characteristics have not been tied together under a unifying principle which could succeed in giving them a hanging-togetherness

and make possible a unified understanding of the total personality. This has encouraged many to consider an authoritarian as simply an eccentric or "crazy person" who is ultimately impossible to understand. But this is not so. Such people have a logic of their own which integrates all life for them in such a way as to make their actions not only understandable, but from their own point of view, quite justifiable and correct. This diversity of single characteristics can be understood only, I believe, by understanding the basic philosophy of the authoritarian person. This basic philosophy I shall call the "world-view."

Like other psychologically insecure people, the authoritarian person lives in a world which may be conceived to be pictured by him as a sort of jungle in which man's hand is necessarily against every other man's, in which the whole world is conceived of as dangerous, threatening, or at least challenging, and in which human beings are conceived of as primarily selfish or evil or stupid. To carry the analogy further, this jungle is peopled with animals who either eat or are eaten, who are either to be feared or despised. One's safety lies in one's own strength and this strength consists primarily in the power to dominate. If one is not strong enough the only alternative is to find a strong protector. If this protector is strong enough and can be relied upon, then peace of a certain sort is possible to the individual.

So we may say in more psychological terms that the authoritarian never loves nor respects other human beings any more than the animals in the jungle can be said to love or respect each other. In the last analysis, the alternatives are to fear or be feared.

Once granted this world-view, everything that the authoritarian person does is logical and sensible. We can easily see this for ourselves, if we can only imagine ourselves to be in an *actual* jungle peopled with *actual* wild animals. Then it is obvious that we must have many or all of the authoritarian characteristics if we are to survive. To speak of love, kindness, coöperation or the like in such a situation would be sentimental and unrealistic, like saying, "Let the lion and the lamb love each other" or, "Let us not be suspicious of the lion, for if we are

good to him we shall see that he loves us," or, "It is just because we defend ourselves that the tiger attacks us."

If the world is actually jungle-like for an individual, and if human beings have behaved to him as wild animals behave, then the authoritarian is perfectly justified in all his suspicions, hostilities, and anxieties. If the world is not a jungle, if people are not completely cruel, selfish, and egocentric, then, and only then is the authoritarian wrong.

THE TENDENCY TO HIERARCHY

This is the tendency to regard most or all other human beings as challenging rivals who are either superior (and therefore to be feared, resented, bootlicked, and admired); or inferior (and therefore to be scorned, humiliated, and dominated). People are ranged on a vertical scale as if they were on a ladder, and they are divided into those above and those below the subject on this ladder.

The democratic person in contrast tends (in the pure case) to respect other human beings in a very basic fashion as *different* from each other, rather than better or worse. He is more willing to allow for their own tastes, goals, and personal autonomy so long as no one else is hurt thereby. Furthermore, he tends to like them rather than dislike them and to assume that probably they are, if given the chance, essentially good rather than bad individuals. We shall give the name "perception and appreciation of difference" to the democratic way of viewing individual differences (in contrast with the authoritarian tendency to hierarchy). Here the stress is first of all on the fact that people are human beings and therefore unique and respectable, and only secondarily upon the fact that they may then be ranked for superiority and inferiority. It is as if they were to say, "All people are human beings, but they vary in their gifts." Let it be understood that it is possible with relation to this appreciation of difference to speak of inferiority and superiority, but we must define these terms in different ways. Of course, the authoritarian also perceives differences. But for him, as Fromm points out, "differences are necessarily signs of superiority and inferiority."

A difference which does not have this connotation is unthinkable to him."

THE GENERALIZATION OF "SUPERIORITY-INFERIORITY"

The authoritarian tendency to classify all other human beings into two groups determined by the relation of superiority or inferiority to the subject is furthermore marked by a tremendous over-generalization, namely, to regard the "superior" or stronger person as superior in everything, as generally superior, and to regard the "inferior" person as inferior in everything (since, in a jungle, strength is the only quality that ultimately matters).

For the democratic individual a judgment of superiority and inferiority tends rather to be specific, realistic, and functional. He refers to a particular quality or capacity rather than to the whole person, *i.e.*, "He is superior because he can do this particular thing well." Furthermore, the measuring stick against which this democratic superiority is measured is sometimes not so much the subject's ability but rather the goals, motivations, and tasks of the individual who is being judged. In democratic superiority or inferiority, let us repeat, the basis for comparison is in relation to tasks and problems to be solved; in authoritarian superiority the comparison is made not in terms of tasks or efficiency, but in over-generalized terms which are related to the subject who does the comparing.

DRIVE FOR POWER

The authoritarian person tends to have a strong drive for power, status, external prestige (since in the jungle, power is so necessary). In extreme cases it can be said that he has a psychological *need* for power which may actually be overtly observed, especially in the person tending toward neurosis (see writings of Adler, Horney, Fromm). Furthermore, this power is defined characteristically in terms of power over people. The person with democratic character structure tends first of all to be less concerned with power, status, and prestige and secondly, to define it characteristically in relation to power over problems and tasks

rather than over people. It is, furthermore, characteristic of the authoritarian individual that if he does have power, he tends to use it primarily to assuage his own psychological needs, that is, in a selfish way, and secondly he tends, especially when challenged, to use it in a hard, cruel, or even sadistic fashion. Conversely it is characteristic of the person with democratic character structure that if he does have power he tends to use it, less for personal needs and more in terms of the needs of the group over which he has power.

The reader may find it useful, as I have, to say arbitrarily that it is the authoritarian who seeks for "power," the democratic person who seeks rather for "strength." Power, used in this way, is the symptomatic expression of thwarting of the person's basic needs for safety, belongingness, or love. The true (unconscious) aim of power seeking is then not power per se, but other unconscious psychological satisfactions which the subject fallaciously hopes power will bring. "Strength" in contrast is used to imply capacity to solve problems external to the subject's psyche, that is, social problems, intellectual problems, problems of the real world.

HOSTILITY, HATRED, PREJUDICE

This is one of the best-known characteristics of the authoritarian person and need not be discussed at length. It is necessary to point out only that its object is, psychologically, an accidental or fortuitous choice. For instance, anti-Semitism or anti-Catholicism, or anti-negroism are none of them theoretically necessary attributes of authoritarianism. What *is* necessary is hatred and hostility against *some* group or other or other, whichever happens to be most convenient. Theoretically it might just as easily be peopled with long ears, or blue eyed people, or poets, or butchers, or bald men. Only hatred for a scapegoat is constant here, not the choice of the scapegoat.

JUDGING BY EXTERNALS AND JUDGING BY INTERNALS.

Another distinguishing characteristic of the authoritarian is that external signs of strength, prestige or dominant status, *e.g.*,

titles, money, power over other people, family name, noble birth, etc., are very important in determining who is superior and who is inferior. In part this judgment of respect comes because of the fact that the ones with prestige have the power of hurt over him. But this is not all there is to it. There seems to be a spontaneous flow of respect and even abasement because of the sheer fact that they are rated as "superior," no matter for what reason. For the democratic person these external signs and marks and symbols are less important than the essential character or personality or capabilities of the person being judged. He is apt to judge by internals rather than externals. (Although of course externals may have some influence also.) It is not enough for him that the person being judged has titles or honors or is a celebrity. He must also be a good human being. Or we may say it another way. He customarily gives his permanent respect only to people who are worthy of respect for functional reasons. He does not give his respect automatically simply because he is supposed to, or because everybody else respects this person.

SINGLE OR MULTIPLE SCALES OF VALUE

In the authoritarian, as we have seen, there is the tendency to have but one scale of values by which to measure all people and all achievements, which is in terms of the personal and social over-generalized superiority or inferiority we have spoken of. In western civilization this inferiority or superiority most often comes on some such single basis as wealth, noble birth, family name or the like (anything that will give power over people). The authoritarian will automatically have the tendency to defer to these superior people no matter what the field may be, no matter what the question at issue, simply because they rank high on the dominating scale of value. The democratic person, on the other hand, will tend to recognize many scales of values and is much more ready to consider scales of value which are different from his own. Furthermore these scales of value tend to be specific and functional. He will respect the man who is a better historian if the discussion is about history, but will not necessarily therefore respect him in other fields in which he is inferior. Furthermore

he is more ready to admire John Doe because John Doe has achieved out of life what he, John Doe, wanted to achieve, *e.g.*, he has become the best chemist or the best baseball player, or the best novelist or whatever, even if no one of these achievements is important for the judger himself.

In addition for the authoritarian every person with a different scale of values tends to be, to some slight extent at least, a threat. For the democratic person this is most often not so. If someone else has different values, he is not threatened thereby.

THE IDENTIFICATION OF KINDNESS WITH WEAKNESS

Consciously or unconsciously the authoritarian will tend to identify kindness, sympathy, generosity with weakness (inferiority) and to identify cruelty, brutality, selfishness, or hardness with strength (superiority). The democratic person has, because of his character structure, no such tendency, or even a tendency in the opposite direction. Courtesy, honesty, and a good many other qualities which we consider to be good, an authoritarian will consider simply to be weak or foolish or degenerate. This is not as arbitrary and senseless as it appears to be. If we go back to our original analogy and understand that our authoritarian is actually living in a psychological jungle, then the lamb who trusts the lion, who believes what he says, who is kind to him, is in actuality an idiot, and such behavior is in actuality dangerous. If we were to grant the authoritarian's postulate that everyone is out for himself, that everyone is selfish and the like, then a man who does trust anyone in the world of thieves is actually someone to be despised as unrealistic.

THE TENDENCY TO USE PEOPLE

It is easy to see from all the characteristics we have discussed so far that the authoritarian will be very apt to regard other human beings only as tools, as means to his end, as pawns on his chessboard, as objects to be exploited. In the extreme we may even detect the tendency to regard inferior people as not quite human so that it doesn't matter so much if they are pained or deprived or exploited or even killed. This too is logical granted the authoritarian's fundamental outlook on the world that one

must kill or be killed. Furthermore, the very act of killing or hurting or exploiting, is in itself, a kind of validation or proof of his superiority and strength.

THE SADISTIC-MASOCHISTIC TENDENCY

It is for most of us in our society, fairly easy to understand that a person should want power. We can with little difficulty understand Mussolini's desire for power (even though our unconscious motivations to power may be different) but it is less easy to understand motivation of the one who gives up his life for Mussolini. How about the authoritarian who is in the subordinate position? We must understand clearly that the tendencies of which we have spoken, have two sides both of which exist in the same person. Every authoritarian character is both sadistic and masochistic. Which tendency will appear depends largely (but not entirely) on the situation. If he is in dominance status, he will tend to be cruel; if he is in subordinate status, he will tend to be masochistic. But because of these tendencies in himself, he will understand, and deep down within himself will agree with the cruelty of the superior person, even if he himself is the object of the cruelty. He will understand the bootlicker and the slave even if he himself is not the bootlicker or the slave. The same principles explain both the leader, and the follower in the authoritarian group, both the slave-owner and the slave.¹

One important distinction is necessary here. We must be careful not to identify all submissiveness or overdependent persons as authoritarian. The slave with a kind master, the dominated over-protected person, the person who has low-self-esteem even though he is secure, the sheltered person or the cowed person, may all be submissive and dependent and *not* masochistic or authoritarian. With such people our analogy of the rabbits and the lions does not hold. If we must pursue an analogy, we should have to speak of a shepherd and his flock of sheep, rather than the jungle with its weak and its strong animals (see also

¹ See Jerome Weidman's story *Chutzpah*, about a man who is admired and envied by his neighbors for being "successful," ruthless, "strong," and clever, even though it is they who have been hurt and outwitted by him.

Fromm, *ibid.*, pp. 174 ff.). These passive followers, who are however, not basically authoritarian, must constitute a sizeable percentage of the population in both the "fascist" and "democratic" countries.

THE POSSIBILITY OF SATISFACTION

The authoritarian can because of his nature practically never be ultimately satisfied. He must go on and on and on. The overt need for power is of course insatiable because the only theoretical satisfaction would be to have complete power over everyone in the world and even then one could be threatened by the resentment of slaves, the lack of friends, the inability to trust anyone, and of course also by the biological exigencies of life—illness, old age, and death itself. The authoritarian must be perpetually and insatiably ambitious. This means also that he can never be happy except for a time. For the democratic person this is not true. He can be happy because his most basic needs are satisfied. Moreover, the satisfactions he seeks are attainable, whereas the satisfactions the authoritarian seeks are unattainable, even theoretically.

We can see further how this must be so if we recall that the authoritarian seeks power as a means to unconscious ends. His basic ungratified needs are for safety, belongingness, and for love. And since authoritarian power rarely attains these ends but rather is more likely to achieve even further frustration of these needs, the need for power must usually be unsatisfiable.

And yet it is possible to differentiate between relatively "adjusted" and relatively maladjusted authoritarians. So long as nothing contradicts their world-view and so long as they have power enough to protect themselves, they may be relatively contented. Certainly such a man will at least be *more* contented than if he had no power at all.²

GUILT FEELINGS AND CONFLICTS

In Western civilization, there are strong cultural forces that

² See for excellent illustrations, the novel, *What Makes Sammy Run* by B. Schulberg, the moving picture, *Citizen Kane*, and the play, *You Can't Take It With You*.

foster both authoritarian and democratic characters, *e.g.*, capitalism, nationalism, militarism, authoritarian education, the patriarchal family, etc., versus the Christian ideal, humanitarianism, socialism, cooperative movements, etc. Intra-psychic conflict is therefore practically inevitable for the average authoritarian (unless he has been brought up under a consistently authoritarian culture from his earliest days). As might be expected he tends to re-interpret all the pressures at odds with his philosophy, forcing into the democratic forms an authoritarian content. (See, for instance, how often the Christian ideal has been corrupted and perverted into its very opposite by various churches and other organized groups.)

Such interpretation is, even when it is relatively successful, a Procrustean task, not to be achieved without great effort, strain and repression.³ I have found in most of my cases (not all) strong guilt feelings, sometimes quite conscious. These guilt feelings, as we should expect, are an additional source of the hostility feelings and impulses that are characteristic of the authoritarian character in this country.

OTHER CHARACTERISTICS

This description could clearly be carried on and on indefinitely. Because of limitation upon space, we need do no more than mention certain other tendencies of the authoritarian character; for fuller discussion, the reader is referred to Fromm's book, as well as to Hitler's *Mein Kampf*, still the best source book available.

1. The abyss between males and females. The tendency to dominate women because they are weaker, and to assign to them a lesser rôle. With this goes the tendency to divide all women into Madonnas and prostitutes, the former being good but non-sexual, the latter being sexual but bad. Another aspect of this tendency is the exaltation of masculinity and its sharp redefinition in terms of power, hardness, cruelty, etc., and the use of sex and "love" as a power mechanism.

³ For an example, see Koestler, *Darkness at Noon*.

2. The development of homosexuality.
3. The soldier-ideal. Ambivalent attitudes toward death.
4. The rôle of humiliation in an authoritarian world. Its function as a validation of status.
5. The antagonism to education particularly of the "inferior" ones.
6. The tendency to avoid responsibility for one's own fate.
7. The concept of *ecstatic* submission, of *eager* giving up of independence to some stronger protector.
8. The authoritarian's achievement of a pseudo-security through compulsive routine, order, discipline, fixity, and other compulsive-obsessive mechanisms.

CLOSING NOTES

Is it possible to change the authoritarian person? We can say "yes" with the utmost assurance, for this change has been wrought many times by psychoanalysis and by shorter therapies as part of their routine psychotherapeutic business. But this is only a partially practicable answer, for these people come to be cured, not of authoritarianism but almost always of specific neurotic or psychosomatic symptoms. Where there is no will or desire to become well, cure is very difficult.

There remains the final question, "Is the authoritarian ultimately right or wrong?" If we confine ourselves to purely psychological considerations the answer is easy. The conditions which the authoritarian attributes to human nature in general are in point of fact found only in a small proportion of our population. The only individuals who ultimately fulfill their conditions are those we call psychopathic personalities. Of no other human beings can it be said that they are completely selfish, completely ruthless, completely without conscience, completely without basic ties or self-imposed obligations to other human beings.

PART III

ANIMAL PSYCHOLOGY

THE EXPERIMENTAL EMBRYOLOGY OF MIND ¹

LEONARD CARMICHAEL

Tufts College

I. INTRODUCTION

A human spermatozoan is a tiny, free, human organism. Its behavior is determined by the manner in which its living protoplasmic system reacts to the chemical, mechanical, and other energy relationships of its environment. It may be said that the movements of the sperm are chemotactic or rheotactic. When such a proper motile and mechanistically determined sperm fertilizes a proper human ovum in a suitable environment, processes start which sometimes lead to the development of a new and infinitely complex, but again freely moving, human organism. Certain of these organisms, so developed, at definite stages in their lives perceive, have emotions, reason, talk, build airplanes and tanks, and bomb other similarly constructed human organisms.

How do these big children of the human ovum and sperm come to have the mental capacities of adult men and women? In the growth from the fertilized immobile egg to voting citizen is there a point at which the new organism throws off the old mechanistic dependence upon the environment which held the movements of its father, the sperm, in rigid grip? This is a problem which has long been recognized, but which until recently has been answered mainly in terms of prescientific verbal speculation.

This paper, then, is devoted to a consideration of something of the present status of what used to be called the "original nature of man." In this treatment especial attention will be given to the

early growth of the mind and to some guesses concerning the relationship of a study such as this to psychology in general. The functional study of the early growth of the animal body has been termed experimental embryology. By analogy, I have chosen to discuss in this paper the experimental study of the early development of those processes which are fundamental to man's mental life. This field I shall call *the experimental embryology of mind*.

It is fashionable, and wisely so, to present the presuppositions which are accepted before beginning the discussion of a fundamental psychological problem. It may be well, therefore, to say that in this paper it is taken for granted that, if any two living systems were ever absolutely identical in every physical and chemical respect, they would behave in exactly the same way under identical conditions.

Adherence to this mechanistic position makes it appropriate at once to ask certain other fundamental questions, if one is to study the growth or disintegration of the living organism. In the first place, what has been called the error of potentiality must be recognized and, save at times as a verbal convenience, avoided. This means that, scientifically speaking, we must not say that there is an oak tree in each acorn. The tree is not there, and poetic falsehood is not science.

To say, for example, that the cells of the developing optic cup are being formed so that the organism may see roses in its later life is to confuse what Aristotle called final cause with material or efficient cause. In other words, in our consideration of growth it is assumed that the only necessary conditions of phenomena are those events which precede in time the stage of development under consideration. In the growing organism it must be emphasized, however, that the whole past of the race as represented in the chromosomal and general germinal basis of heredity must be included in the temporal past.

In 1754 Étienne Bonnot, Abbé de Condillac, adopted in his treatise on sensation the artifice of the supposition of a statue with marble exterior whose sense organs are awakened from their sleep successively (21). Condillac, in warning his readers against what has just been called the error of potentiality, says:

I wish the reader to notice particularly that it is most important for him to be himself in imagination exactly in the place of the statue we are going to observe. He must enter into its life, begin where it begins, have but one single sense when it has only one, acquire only the ideas which it acquires, contract only the habits which it contracts: in a word, he must fancy himself to become just what the statue is. The statue will judge things as we do only when it has all our senses and all our experience, and we can only judge as it judges when we suppose ourselves deprived of all that is wanting in it.

Let us follow the good Abbé's admonitions, avoid the error of potentiality, and look at the immobile mammalian fetus before its moment of animation or quickening. Let us then see how its behavior changes as its nerves and muscles develop and as its senses gradually become able, when properly stimulated, to initiate changes in the activity of the whole living protoplasmic system which we call an organism.

II. THE ZERO OF BEHAVIOR

First of all it is important to remember that, at all stages, the fertilized germ cell, the embryo, and the fetus are alive and existing in an external environment. The dynamics of cell division, cell differentiation, and cell migration, and the dominance of certain cell groups over other cell groups occur and can only occur in this living, growing organism when the external environment has fixed limits of variability. That is, an environment which is organism-maintaining must have quite specific chemical, thermal, pressure, and other characteristics. Internal to the growing organism there are also fluctuating electrical, chemical, and mechanical relationships between cell groups which are basic in the growth of tissues and organs. Some areas of each complex growing individual have relatively rapid metabolism, others relatively slow metabolism. Moreover, in certain regions of the growing animal rhythmic cell movement begins early. In the human fetus, at about three weeks heart cells begin a rhythmic but faltering beat which will necessarily change and develop but still in some manner continue until that individual organism is legally pronounced dead (55).

Thus our statue, the fetus, is far from the dead marble of

Condillac's imagination in the period before it first moves as a result of external stimulation. In its complex system of systems, an equilibrium is always being regained and in turn upset. In such alterations growth may be considered as a continuous shift of the baseline to which the restorations of the equilibrium of its life processes must ever return.

If we wish to place a zero point of mind at any level, in spite of the continuity of organic processes, it may be set at the first moment when the living system reacts to an external energy change so as to alter the external relationships of the system. Unfortunately, this zero point does not seem to come at the same place in the continuity of development in all organisms. A pretty and accurate picture has been painted of the early behavior of young fish embryos by Tracy (51). Without functional external receptors these organisms are impelled to action by changes in their own internal environments. That is, by changes in the amount of oxygen available in the blood and by increase of metabolites in the blood, the central nervous system of such motile embryos is directly affected (2). Motor peripheral nerves as a result are activated, muscle movements take place, and acts of locomotion occur. These first responses are fully blind, but still by means of them the reorientation of the total organism in relation to its environment is accomplished. Such activity may well move the animal so that once again a proper supply of oxygen in the surrounding water is available for its needs. Gradually, in such fish embryos, the senses become functional, and the external control of behavior emerges, or, rather, here the external world through the external senses captures a precarious dictatorship of the organism. Such a Schopenhauer-like picture of the primary growth of the pure motor response and the subsequent development of control by the external world seems established in the fish embryo. As the investigator himself puts it: "The animal is driven through its environment as a result of stimuli which arise periodically in connection with its own metabolic processes." Thus we see a whole organism beating, as it were, in its outer environment as previously its heart cells beat in their own cellular environment.

Unfortunately for the esthetics of science, this pretty sequence cannot be found in overt form in all species, but the picture which it presents is instructive. Organisms differ from species to species in regard to the temporal sequence of functional development of the elements of the response mechanism. Muscles may be directly stimulated to response before neural control is established. Those who like analogies may look back from embryology to the sequence of phylogeny as proposed by Parker (42). According to this investigator, the independent effector or primitive muscle is the first specialized element of what is eventually to become the mammalian response mechanism. Only later in phylogeny do specialized receptors and nervous system develop and assume control of the more primitive contractile systems. The immature muscles even of mammalian fetuses act as independent effectors before neural control is established. Minkowski, for example, has shown that the first reaction of the human fetus to stimulation on the sole of the foot is ideomuscular—that is, a direct muscle response (37).

The fact that muscles can thus be directly stimulated has made it difficult in certain instances to determine the exact time of onset of true externally excited behavior. Windle and Griffin describe a twitch of the forelimb of the cat fetus brought about by stimulation as a true reflex—that is, as a response involving external receptor, central nervous system, and muscle action (57). Coghill and his associates, on the other hand, possibly because their work has been done so largely in the amphibian field, believe that a response of the type described by Windle may well be an independent muscular response (23, 24). In regard to this specific point, however, it may be said that Barcroft and his associates, working on the large fetus of the sheep, have confirmed the fact that the first response of this animal is of the nature of a neurally mediated reflex act (3). Because of the significance of this problem for all later investigations of the embryology of behavior, Bridgman and I have studied a large number of fetal guinea pigs, carefully dating the gestation period so that we would have litters at about the hours of first motility (5). We made moving pictures of the initial response of many of these

fetuses. On the basis of this work we were able to conclude, with some assurance, that, in the guinea pig at any rate, the first response that can be elicited as the result of stimulation is a relatively localized one involving the lateral flexion of the neck and the sometimes simultaneous extension and later flexion of a forelimb. By a series of controls we convinced ourselves that this movement was a true reflex-like reaction and not a direct muscle response. This conclusion seems to have somewhat wide implications.

Coghill's painstaking studies of amphibian larvae made it possible for him to make a generalization concerning the early development of behavior in amblystoma which has been called the *Coghillian Sequence* (22). Briefly, this sequence shows that in amblystoma the first response is an ideomuscular one and the second a response of the total organism with a lateral flexion forming a C or reverse C. In the next stage it is noted that before the first flexion has been completed a new one on the other side begins. Thus the S or sigmoid movement develops. As this movement becomes more rapid it produces locomotion and is called swimming. In the salamander all this takes place before the limb buds develop. As the limbs grow, this sigmoid movement is well developed and so it is basic to the timing of the at first passive movements of the appendages. Thus the sigmoid movement is fundamental in the development of the walking, as well as the swimming, rhythms. It should be noted, however, that the first movement of amblystoma occurs, unlike the condition found in the mammalian and human fetus, before the limb buds have developed. Hence Bridgman's and my observation that the first response of the fetal guinea pig is reflex in character does not in any way contradict Coghill's findings on amblystoma. Our results do suggest, however, that the Coghillian formula probably cannot be applied in all its details to the guinea pig and to certain other mammals which we have studied. In these mammals the limbs are well developed before the first stimulus-aroused behavior takes place.

There is much further evidence on this matter to which reference cannot be made here. Again it seems that empirical findings

mar the esthetically attractive picture of the universal formula which would suggest that all behavior begins with the total organism and that reflexes appear as individuations from this total mass response. Above all it must be remembered that interspecies comparisons in behavior are difficult to make without scientific error. In this connection the work of Kuo (33, 34) and of Orr and Windle (41, 60) on the chick embryo should especially be considered.

As yet the facts concerning the zero of receptor-controlled motility in man are not clear. It is difficult to secure human material for such studies. Minkowski (38), Hooker (29), and a few others have studied the responses of such early human fetuses as have been available to them. Unfortunately, in almost all this work the observers have been forced to deal with dying organisms. Ordinarily the human fetus can be experimented upon only after the interruption of its placental blood supply. This means that the study of the behavior of the excised fetal organism, while interesting and extremely significant, is probably not typical of the behavior of human fetal organisms under normal conditions. My own experiments on the cat, rat, and guinea pig fetus, for example, show that even momentary interruption of the placental blood supply alters behavior in a most striking way. This fact is demonstrated clearly in an experiment carried out by Jasper, Bridgman, and myself (30). We are able to demonstrate that the electrical activity of the late fetal brain as shown in a fetal electroencephalogram was almost instantaneously abolished by tying the umbilical cord of the fetus so as to interrupt the supply of freshly oxygenated blood passing to the fetus from the placenta, where gaseous interchange with the maternal blood stream takes place. Following the tying of the cord, however, the general body responses and reflexes of the fetus continued for some time in an unabated, and indeed in what seemed to be a definitely augmented, form. This observation merely confirms in a somewhat different way a whole series of quantitative studies by Barcroft and other investigators on the importance of a constant oxygen tension in the internal environment if behavior is to be studied comparatively (2). Therefore, in the case of the human excised

fetus it is not possible to say with assurance that the first response is either reflex in character or one involving the total organism. All evidence considered, however, it seems to me that the first response of the human fetus is probably one involving a limb or other segment of the organism. Certainly, I am not convinced that the first human response is a so-called total body movement.

III. THE SENSES IN FETAL LIFE

As already pointed out, the life processes of the fetus depend upon its own cell systems and upon the external environment in which it is growing. When the receptor systems become functional they mediate, as in the statue of Condillac, the special energy relationships between the fetus and its world. The ordinary physiological and psychological methods used in the study of the receptors cannot be applied in the study of the senses during the prenatal period. Methods which demand a verbal or symbolic response on the part of a coöperating human subject obviously cannot be used on a fetus. The employment of conditioned-response techniques and the use of various types of discrimination apparatus which involves learning are, in general, ruled out. So far only one general method has yielded satisfactory results in the study of the senses in fetal life. This method depends upon the initiation or alteration of receptor-induced effector responses which involve the so-called inborn reflexes of the organism. Motion-picture recording of such responses has been found to be a valuable aid to the accuracy of such reports. The use of the techniques of the electrophysiologist has also shown something directly about the growth of functional effectiveness in the receptors themselves and in the neural mechanisms associated with receptors. Electrical techniques have been useful in recording certain temporal and other characteristics of the muscle movements elicited by stimulation.

In this paper I shall not attempt to bring together in detail all that is known about the senses in fetal life; especially, the mass of material summarized by Preyer (45), the greater pioneer in this field; will not be referred to. I have tried to evaluate this material, including recent additions, in previous publications (10,

15). Here I shall, rather, attempt to limit myself to a description, and especially an evaluation, of some of the work on the fetal senses with which I have firsthand acquaintance.

IV. CUTANEOUS PRESSURE

Because it appears so early, the first sense field that we shall consider is the cutaneous-kinesthetic complex. Coronios (25), assisted in certain parts of his experiments by Schlosberg and by me, showed in a large series of litters of fetal cats of accurately known insemination age, that at about the twenty-third day after insemination (the normal gestation term of the fetal cat is 62 days) bending in the cervical region and foreleg flexion can be observed following cutaneous stimulation. Almost certainly such stimulation at this early age also involves kinesthetic receptors. In the work of Coronios, stimulation was ordinarily given by means of light brushes and blunt glass rods. Verbal records of behavior description were dictated at the time and moving pictures taken. As a result of these studies, Coronios showed, possibly more clearly than had been demonstrated before, that there is a continuous development of the behavior released by stimulation of the cutaneous receptors during the whole fetal period. From the first observed twitch there is an unbroken continuum of growth in the effectiveness and in the complexity of elicited responses in the fetal kitten. Windle, Griffin, and others have also made a very careful and illuminating study of the growth of behavior in the cat (57, 59).

To summarize the condition in the cat: At first, in response to tactile stimulation there are mere twitches; just before birth the responses of the fetus are coördinated and adaptive. Reactions that we call swimming, crawling, and defensive movements are easily elicited in the late fetal period. Such responses are as purposeful as any behavior which is unreinforced by language. In these experiments, as in the others with which I have been associated, placental circulation is maintained in each fetus during experimentation. The observations on the fetus are made under a bath of physiological saline solution held at blood temperature by an outer water bath which is thermostatically held constant by heating units.

Raney (46) and Lincoln (36), also working in the Brown University Laboratory, repeated on the rat fetus some of the work just described as done on the cat. The development of behavior in the rat fetus has also been studied by Swenson (50), by Angulo y González (1), and by Windle and his associates (58).

I have carried out a prolonged series of studies on the development of behavior released by all the senses during the fetal period in the guinea pig. In this work through the years I have been assisted by a number of colleagues and especially by my wife. More than 100 cutaneous pressure areas or reflexogenous zones have been investigated in a general study of the fetal guinea pig during its entire active prenatal life of 68 days.

The importance of quantified stimulation in such studies cannot be overemphasized. Smith and the writer (18), using a series of especially prepared Von Frey esthesiometers, were able to show the effect of different pressure stimuli upon the behavior of the fetal guinea pig at typical periods of development. *This study made it clear that from the first, stimuli, just at the threshold, tend to bring out responses which are characteristic of the point stimulated, but that more intense stimuli lead to a wider spread of responses sometimes involving almost all the muscle groups of the organism.* There is also a quantitative relationship between the intensity of the stimulus and the magnitude of elicited movement (14).

In regard to tactual stimulation in the fetal period in various animals the following conclusions may be drawn: (1) The place stimulated is most important in determining the character of the specific response elicited. (2) The age of the fetus influences the generality or specificity of response to some extent, but, if other factors are controlled, it seems that each reflexogenous zone has, as it were, a preferred reflex or pattern of response which is released whenever that zone is lightly stimulated, and this pattern remains very constant from the time it first appears to adult life. (3) The intensity of the stimulus is important both in the speed of the elicited response and in the spread of the response to narrow or wide motor outlets.

V. TEMPERATURE SENSES

Lehner and the writer (17) studied the behavior released by thermal stimuli in the fetal guinea pig in a situation comparable to that just described for pressure stimuli. We arranged an apparatus in which there was a series of vacuum flasks containing liquid of known and graded temperatures. Each flask contained a pipette. By stimulating the animal with drops of liquid at the temperature of its own body, a control for the tactual component of the drops of liquid was instituted. Using this procedure we were able to demonstrate that, as we moved to temperatures below the physiological zero of the organism or above the physiological zero of the organism, the intensity and the spread of the resulting response increased. The fundamental relationships of specific reflexogenous zones and specific forms of behavior were borne out in this study. Here again a quantitative relationship between intensity of stimulus and magnitude of movement was established.

VI. CUTANEOUS PAIN

Surprisingly enough, our investigations show, as do certain previous observations by others, that a sharp needle is in general no more effective a stimulus, so far as the eliciting of behavior is concerned, in the fetal period than is the application of a blunt pressure stimulus such as a hair. The epicritic-protopathic theory of cutaneous receptors suggests that, in evolution, pain is more primitive than light pressure. The experiments which we have carried out give no support to this theory in ontogeny.

VII. THE PROPRIOCEPTIVE COMPLEX

It is difficult to isolate muscle, joint, and tendon receptor fields in the fetus. Coghill has said in the case of amblystoma: "The limb is able to respond very precisely to stimuli arising within the body (proprioceptive) as the result of a particular posture before it can respond to stimuli that arise exclusively from the outside world (exteroceptive)" (22). This observation is probably true in mammals as well. Certainly there is no sure evidence that this state of affairs does not also exist in the fetal

cat, guinea pig, or rat. I have never, however, been able to eliminate to my satisfaction cutaneous components from kinesthetic stimulation in the early period of fetal life in the types which I have studied. I have attempted to elicit the tendon reflex in the fetal guinea pig, but without sure success. It is reported by Minkowski (38), however, that such reflexes may be elicited in the human fetus. The so-called *Magnus Reflexes*, the tonic postural changes of the organism brought about by the bending of the head in relation to the trunk, and so forth, can to some extent be elicited in the fetal guinea pig. Such responses are more adequately elicited in late fetal organisms in which the higher brain centers have been operatively removed.

VIII. THE NONAUDITORY LABYRINTH

Righting responses in the case of organisms which are in contact with a pressure surface can be observed in prematurely delivered fetal cats and fetal guinea pigs, as well as in other mammals. The maintenance of the upright posture when supported only by the liquid in the experimental bath is also observable in these fetuses. But this cannot be attributed to the non-auditory labyrinth alone. The extent to which such responses are tactually and kinesthetically determined is not known. In the same way, the part played by vision in such postural righting is not easy to determine at certain late fetal periods in the guinea pig. The tension of the umbilical cord also may play a part in these responses during the period of free swimming in the bath of warm saline solution.

It is possible to study the genetic development of the air-righting reflex by high-speed motion-picture photographs (40, 56). Keller and I have done this in newborn kittens (11). Warkentin and I have repeated this work on kittens, rabbits, and guinea pigs (53). As a result of these studies there can be no doubt that the guinea pig is born with a relatively high capacity to right itself in the air when falling and thus to land on all four feet no matter from what position it is dropped. Interestingly enough, this capacity is by no means absent in prematurely delivered guinea pigs several days before normal birth. Hence,

by inference it may be said that in the fetal stage the requisite mechanism for this response has sufficiently matured to make it function effectively. The situation is quite otherwise, however, in the cat and rabbit. A newborn cat falls through the air without any tendency to right. The maturation of this capacity, however, occurs rapidly during the early postnatal period. It may be interesting to note that this response begins, in all animals studied, in the head region, and duplicates to some extent in its gradual perfection the sequence of responses by means of which the air-righting reflex is brought into operation each time it occurs in any adult mammal. As I suggested some years ago, the shortening of the time sequence in the perfection of this act may be a developmental principle of wide general significance. Experimental controls instituted by Warkentin in the study of this reflex make it seem almost certain that the growth in the capacity to air-right is not at all a result of learning or conditioning. It is a function of the gradual inner growth or maturation of the receptor-neuromuscular mechanisms involved. This maturation occurs at different rates in different species of animals and also in different individuals of the same species, but we have no evidence that practice is important in its perfection.

IX. THE ORGANIC SENSES

Little experimental work has been done upon the organic senses in any fetus, although studies have been carried out on the movement of fetal intestines (4, 67, 62). Pfaffmann has been able to show that in prematurely delivered cats and in newborn kittens and guinea pigs the sucking of milk is a function to some extent of the fullness or emptiness of the stomach (43).

X. SMELL

No satisfactory experimental work has been done on the sense of smell in any fetus. I have attempted a few casual experiments in this connection, but none of them is worthy of description here. In the human prematurely delivered infant there is some evidence that the individual may avoid a breast covered with kerosene but take one lubricated with odorless oil of the same viscosity (44).

XI. TASTE

Pfaffmann devised an apparatus by means of which it is possible to record graphically the sucking reactions of a newborn kitten (43). By a system of stopcocks it is possible to alter the character of the milk being sucked. Thus taste stimuli may be introduced at known times. If an alteration appears in the sucking curve at the time the new substance is introduced, it can reasonably be assumed that the receptor mechanisms have been activated as a result of the novel stimulus. The conclusion of this study indicates that there is a distinction between sweet on the one hand and sour, bitter, and salt on the other. No sure distinction between the three classes of so-called noxious taste stimuli, however, has been secured in the experiments which have so far been carried out. These experiments are essentially of a preliminary nature.

XII. AUDITION

Kussmaul years ago said: "Of all the senses, that of hearing sleeps the deepest" (35). So far as fetal mammals are concerned, however, this is only relatively true. Rawdon-Smith, Wellman, and I (47) have been able to demonstrate by the use of suitable amplifiers and oscillographs that air-borne auditory stimuli are effective in eliciting the so-called *Wever-Bray Effect* or electrical cochlear response on the same developmental day in which the first overt behavior to auditory stimuli is brought about in the fetus of the guinea pig. That is, in a fetus 16 days before normal birth-time a small electrical response of from one to two microvolts in peak can be obtained to an air-borne auditory stimulus of 600 cycles per second, at a relatively high intensity. Decreasingly great electrical output is obtained to tones below this and above 2000 cycles per second. The voltage of electrical potentials so elicited increases rapidly as fetal maturity progresses, so that by six days before the normal birth-time an electrical response of at least 100 microvolts can be recorded. This experiment gives a pretty illustration of the usefulness of electrical recording in connection with receptor activity itself.

XIII. VISION

Work by Coronios (25), Warkentin (52, 54), and others with whom I have been associated has shown in several species of animals some of the facts of the onset of visual capacity during the fetal and neonatal periods. This is a difficult field in which to work. There is very great difference in the readiness of the visual mechanism to function at the time of birth in different species of mammals. The guinea pig's eyes are open, and light stimulation can be shown to be probably effective in the fetal period on the fifty-sixth postinsemination day. In the cat, on the contrary, as is well known, the eyes do not open until a number of days after birth, a period varying, in Warkentin's observations (54), from 3 to 15 days. The motor mechanisms about the eye also mature slowly, but they can sometimes be brought into play before visual stimulation is effective. Eyewinks can be elicited to tactual stimulation of the lids or cornea before such responses are brought about by photic stimuli. The first eyewink to be called out by tactile stimulation in the guinea pig fetus occurs in the still unopened lids at 35 postinsemination days (12). At this time there is also a slight change of the total eyeball in the orbit, apparently as a result of the contraction of all the external eye muscles. Evidence is also developing in experiments which Wellman and I are conducting at the present time which suggests that eye movements can be elicited by the proprioceptive stimulation caused by changing the body in space before they can be elicited by any form of light acting as a retinal stimulant. In some of this work we have availed ourselves of the well-known phenomenon that passing striations of light over the eye of an organism may, when conditions are satisfactory, set up compulsory, reflex, optokinetic nystagmus. Using these inborn and highly integrated responses, it is possible to study the onset of certain aspects of visual capacity in the fetus in an exact and quantitative way. In order to record the fetal eye movements we have found, as reported in a still unpublished report, that the electrical recordings of shifts in the corneoretinal potential from the small eyes of unborn guinea pigs can be made with excellent results (16). Our preliminary conclusions indicate that,

at least from 60 postinsemination days onward, such visually released nystagmus to moving striations can be elicited and recorded in the fetal guinea pig. Hence it is possible to say that vision of a measurable acuity develops before birth in this animal.

Using a similar technique but varying the striations used, Warkentin has made elaborate studies of the development of visual acuity in newborn and prematurely delivered cats, rabbits, guinea pigs, other mammals, reptiles, and amphibia (52). These results show in general that in ontogeny there is a gradual development of pattern vision—that is, the capacity of the eye to respond to sharp differential gradients of light. This change in the latter part of the fetal period and in the early part of the neonatal period is a function of a number of developmental changes in the optic media of the receptor and their associated neuromuscular mechanisms. The maturation of the retina, the optic nerve, and optic centers is important in the perfection of these responses. Incidentally, in this work some evidence suggests that the vitamin content of the diet is very important in determining the onset of acuity vision, and, since radiation is important in diet, care must be exercised in comparing the effectiveness of eyes of animals reared in the dark and those reared in the light. In certain experiments which allege that light stimulation and, inferentially, learning are important in the early development of effective vision, this factor has not been controlled. Mowrer, however, has shown that exercise is not without its effect in the development of this mechanism (39).

The technique described above has been adapted to studies of normal and premature human infants. Because of the intimate relationship between Vitamin A deficiency and night blindness, it has been found possible to use this technique as a sensitive diagnostic aid in connection with tests for Vitamin A deficiency in newborn babies.

In what has just been said about the development of the cutaneous, auditory, visual, and other senses it has been necessary by inference to refer to the growth of motor effectiveness. Now, however, we may, it seems, advantageously turn from our statue's senses to its behavior repertory itself.

XIV. MOTOR DEVELOPMENT

Coronios, on the basis of the study of many litters of fetal cats mentioned above, was able to draw up a time schedule for the expected appearance of specific responses in that animal (25). In the comparable study of the fetal guinea pig over 100 areas and receptor fields were selected which were systematically stimulated in a series of fetal litters of known postinsemination age. In this study 96 different litters were used. Of course, it was not possible to stimulate each point on the youngest fetuses, but the whole series of zones, or as many of them as possible, was studied in each fetus used (12). Motion-picture records and dictated protocols were made of each experiment. At the conclusion of this particular experimental series, which occupied many months, the different protocols were recopied in relation to the area stimulated. Then for the first time what seems to me a remarkable fact emerged. In spite of some masking at times by general activity, there appears to be a *pattern of behavior which is the characteristic response of virtually every exteroceptive point stimulated on a fetus*. As noted before, when stimuli much above the lower threshold are used, this pattern is often masked by general behavior. Similarly, when other stimulation, as from the pressure of the supporting surface, acts on the fetus, other behavior than that typically elicited by the applied stimulus may take place. But in general each stimulus point or cutaneous reflexogenous zone, when optimally stimulated, releases a pattern of behavior which is remarkably constant from the first time it appears in early fetal motile life until birth and, indeed, until adult life. For example, when a stimulus is applied to the upper lip just to the right or left of the midline on the snout of a guinea pig fetus of 50 postinsemination days, very specific behavior results. In such cases the paw on the stimulated side is almost invariably brought to the point of stimulation. If the stimulus is moved a millimeter from one side of the midline to the other, the other paw is at once brought into play. This same reaction can be demonstrated with other types of stimuli such as drops of warm or cold water, but not necessarily by drops of water of the same temperature as the fetus. Thus it

is not the physical character of the stimulus, but rather that it shall be above the threshold of some of the complex of skin receptors and in a specific locus, that determines the response. Such typical patterns of behavior remain amazingly constant in an organism that is rapidly growing, and, conversely but similarly, growth may suddenly alter such responses, and such alterations of behavior may easily be confused with learned responses, especially in postnatal life.

This description may suggest what has been called behavioral atomism. If so, the fetus, not the investigator, may be blamed for the sin. The statement just made does not mean, however, that adult behavior or integrated behavior at any age is any mere tying together of discrete patterns of response. Of course, a "bundle hypothesis" in this sense is not a true description of the facts of growth. In the first place, adaptive responses are seldom elicited by a series of pressures given in special temporal orders, as it were, upon a series of cutaneous doorbell buttons. A chain of central processes and temporally discrete proprioceptively and exteroceptively aroused events is involved in such responses. Nevertheless, I have never seen any responses in the late fetus which, in their elements, have not appeared as a typical patterned reaction to isolated stimuli many times before. In the late guinea pig fetus the hair coat is well grown, the teeth are erupted, eyes and ears are functional, and adaptive integrated behavior is well established. At this time such an animal will, to use the language of teleology, attempt in a most effective and even ingenious way to deal with a tactual stimulus applied to its lip. First, it may be, it will attempt to remove the stimulus by curling the lip; then, if the stimulus remains, it is brushed by the forepaw on the stimulated side. If the stimulus still persists, the head is turned sharply. Finally, a general struggle is resorted to which involves movements of all four limbs and all trunk muscles. In a late fetus this final maneuver is sometimes so quick and effective that the experimenter is often thwarted and the offending stimulus is removed—by a guinea pig fetus that is having its own willful and annoying way in spite of anything the experimenter can do. Each of these special responses, however, may be seen as an old

one to the person who has watched the growth of fetal behavior. Moreover, it does not seem to be an aid to understanding to say that such purposeful and effective behavior is the response of the organism as a whole. Rather, it can be said that this behavior illustrates the fact that the course of fetal development is the story of the initial appearance and then the continued maintenance of a wide variety of specific mechanisms. The timing and interplay of these mechanisms make the admirable machine of the tiny body able to adapt itself in many varying and successful ways to environmental changes. Thus, mechanistically and, I believe, without environmentally determined learning these little machine-like organisms grow more effective as they become older. Complex patterns of behavior emerge as a result of maturation. Such behavior is possibly as truly end-seeking and purposeful as is any behavior in the world which does not involve the use of language. I see no reason to believe that this emergent purposeful behavior is not as natural a result of the processes of growth as is the length of the fetal whiskers, and quite as independent of learning.

The growing animal functions in a way that is in general adaptive at every stage. When I wrote my first papers in this field, dealing with the development of drugged amblystoma (6, 7, 8, 9), I was so under the domination of a universal conditioned reflex theory of the development of adaptive responses that I denied categorically the truth of the statement just made. But every experiment that I have done in the field of the early growth of behavior has forced me to retreat from this environmentalist hypothesis. Now, literally almost nothing seems to me to be left of this hypothesis so far as the *very early* development of behavior is concerned.

XV. GENERAL IMPLICATIONS OF THE STUDY OF FETAL BEHAVIOR FOR PSYCHOLOGY

Besides providing a genetic description of the growth of sensory-controlled behavior and behavior which external observers call purposeful, there are also other implications of the study of fetal behavior for psychology. Indeed, in general it may not be too much of an exaggeration to say that the experimental

embryology of mind may provide a basis for the psychology of adult human mental processes which is comparable to the basis for adult anatomy provided by a knowledge of the morphological embryology of the human organ systems. For example, one of the oldest and most insistent puzzles concerning man, or, if you wish, our adult statue with all its senses and nerve centers, is that many stimuli which are very diverse as physical energies release behavior which is identical, or nearly identical, in the living individual. Likewise, the energies of stimuli which are very similar or even alike in the quantitative descriptions of the physicist sometimes release patterns of response which are quite dissimilar in different organisms or in the same organism at different times.

How is deterministic experimental psychology to deal with this problem? It seems that other natural scientists sometimes look with suspicion upon all psychology because this embarrassing question exists, not alone because it is not answered to their liking. May psychology remind such scientific friends that because the living organism does not act as some simple machines does not disprove a mechanistic approach to psychology? One can well imagine an old-fashioned mechanical engineer looking at a steam turbine and saying that the new device cannot be a steam engine since it possesses neither pistons nor cylinders. For such a man pistons and cylinders are the essence of steam engines. In a similar way even some psychologists seem to feel that, because of the facts just described concerning the biological and psychological equivalence or nonequivalence of physically identical stimuli, the organism cannot be subject to deterministic rules. Such individuals, it seems, are merely defining the word "mechanical" in a special way. To them it refers not to the basic principles of mechanics but rather, it may be because of a limited past experience, only to certain classes of machines—for example, those which have gears.

Parts of the answer to the question of how it is that dissimilar physical stimuli are similar or apparently identical in their effects on living organisms require, it seems to me, an appeal to the experimental embryology of mind. First let us look at the facts. In recent years Klüver's significant studies (31), and those

of many other individuals, have focused experimental and theoretical attention upon this subject. Klüver has succeeded in showing experimentally, at the perceptual level, that physically heterogeneous situations are sometimes treated as identical by animals. This may be taken as demonstrating the existence at such times of basic behavior mechanisms in the organism which are fundamental to the acts observed. It is my belief that the complete story of the embryology of mind-as-behavior will give the ontogenetic history of such mechanisms, and thus it will become clear that the specific maturation of the receptors and the nervous system determines the identical responses that are made to different types of stimuli as already described above in examples taken from fetal life.

In this connection the stability of patterns of response in fetal life takes on a new significance. The growing anatomical structures of the guinea pig fetus are so organized that a wide range of stimuli, if appropriately applied, call out essentially the same response throughout a long period of growth and, indeed, throughout life. The degree of suddenness of change in a gradient of stimulation affecting a receptor field, for example, may be more important than the absolute physical characteristics of the energies of the stimulus. In understanding fundamental behavior of this sort, too much emphasis cannot be placed upon the real advance in psychological thinking made by the results of the gradient experiments of Köhler (32). While his experiments have been largely limited to the perceptual fields, their implications are as wide as animal behavior.

It is interesting to note that a whole series of characteristics of stimulus patterns, as they are applied to the organism, such as suddenness of onset, cessation of application, movement, and, above all, change in intensity, initiates in receptors what may be called neural signals and little else that is important. These signals or propagated disturbances pass to the central nervous system. A sudden flash of light, a sudden sound, or an appropriately placed sudden tactual stimulus may all call out an eyewink in a late guinea pig fetus. These stimuli are physically quite heterogeneous, but they may be said to be, in one respect at least,

equivalent. Only by empirical experimentation can such families of equivalence be determined. But possibly more important is the fact that only by a knowledge of the developmental history of specific behavior mechanisms can such reactions be made amenable to that sort of scientific description which allows prediction of future specific reactions. Suddenness of presentation, movement of the stimulus, and the like are, of course, those characteristics of stimulus pattern which classical psychology singled out to describe as the determiners of attention. It may be significant for adult psychology that these stimulus characteristics are already effective in fetal life.

To revert to the physiological level, it is important to notice that alteration in the make-up of the fluids constituting the internal environment of the central nervous system may sharply modify behavior. Some of the characteristics of behavior in late fetal life, as contrasted with those of early fetal life, are almost certainly due to the fact that not only has the nervous system matured, but also the brain of the late fetus has less oxygen at its command than has that of the early fetus. This picture is a most complex one. The higher centers of the brain in the case of the late fetus are functionally more active than in the earlier period, and it is just these higher centers which are most subject to oxygen changes in the blood. In the same way internal alterations in temperature, blood sugar, water, and the like allow the external environment to call out behavior which would not be elicited in different internal conditions of the organism.

An interesting example of a late-maturing pattern of behavior which can be elicited by appropriate stimulation of specific reflexogenous zones ordinarily only after sexual maturity, and then only in the brief period of sexual receptivity, once each 16 days, is the copulatory reflex of the female guinea pig. This reflex, fully described a few years ago by Young (64), can be elicited in all normal female guinea pigs during a period of a few hours in length only once during each receptive period of the reproductive cycle. This very definite reflex can be called out by direct tactual stimulation of the reflexogenous zones on the animal's back and flank. This so-called reflex is, of course, in reality

a complex behavioral act involving almost the entire musculature of the organism. As a patterned act it is unlike any other response of the guinea pig, although its elements are old. It appears in complete form the very first time the appropriate stimulus is applied, even in an animal which has been reared alone. Experimental hormonal manipulation of the blood stream of the guinea pig, moreover, will alter the character of this response. Here, then, we have a specific stimulus-neuromuscular pattern which appears only when the chemical make-up of the internal environment has certain very specific characteristics. This late response may then be said to be complicated, adaptive, and yet essentially independent of learning.

The work of Carpenter on the reproductive behavior of the pigeon likewise shows that the condition of the internal environment of the bird determines in a very specific way the repertory of complex patterns of behavior which is typical of each stage of reproduction (19, 20). Such responses may, therefore, without too much error, be called inborn. For example, when nest-building is the order of the day, any little sticks in the bird's visual field are selected with greatest care and piled into nests. As soon as the cyclic chemical clock of the internal environment has passed what may be called the nest-building point, behavior changes. At this time the perceptual and attentional field of the bird alters. Twigs, straws, and other nest-building materials are no longer important; other responses related to egg-laying take their place. Experiments by Stone (49) and by Richter (48) show analogous relationships in the rat. The recent reports given by Yerkes (63) and by Crawford (26) on social dominance and the menstrual cycle in chimpanzees are significant in this connection.

Much of the infinite complexity of the adaptive behavior of the adult organism thus appears to be a result of timing and interrelationships of the patterns of response which are set off by the alterations of the external environment acting upon an organism which is always conditioned by the cycle tides of an ever-changing internal environment. In this connection it is interesting to note that the afferent neural impulses initiated by the external senses

play upon what is today seen clearly in all electroencephalograms to be a continuous rhythmic and changing background of activity in the central nervous system itself (30). In spite of this I am willing to follow Guthrie and Horton (28) when they point out that even in learned problem-solving behavior the activity of adult organisms may be at times as stereotyped as we have seen it to be in fetal life. Thus, given the same set of stimulus conditions, internal and external, the animal's posture, even of the tail, is stable in trial after trial. This evidence suggests that once a pattern of behavior has been established in fetal or later life by growth or by those physiological processes which underlie learning in later life or by growth and learning in coöperation, it will continue to be released without change in a similar stimulus situation. That is, it will so continue until definitely altered by changes in inner or outer conditions. It is clear that even certain language mechanisms of vocal and subvocal speech behave in this way.

This same point of view has interesting connotations for what is called the psychology of needs or drives in their relation to the experimental embryology of mind. The needs of the fetus are few. Oxygen, water, food, and an even thermal environment are provided and regulated by the maternal organism. If one clamps off the blood supply of the umbilical cord in late fetal periods, as noted above, an increase in activity on the part of the fetus is often observed. As scientists, we external observers know in such cases that the organism needs oxygen. In the fetus, however, it is clear that the growth of the nervous system has provided a mechanism which now, the first time it is called upon, makes gasping and air-breathing possible. Here we have a simple and typical example of "need psychology" in a fundamental form. The physiologist knows that the organism must have oxygen if it is to survive. The nervous system of the fetus is so constructed that when its internal environment comes to lack oxygen, already existing neural patterns basic to patterns of behavior are released. These responses may supply this want by establishing air-breathing. The only proviso is that the neural and muscular mechanisms be ready. This means, then, that what the external observer

correctly enough in this case calls the "organism need" is an alteration in the internal environment of the fetal nervous system. As a result of this change, previously inoperative patterns of behavior are activated. In this example, therefore, the word "need" may be abandoned. It adds little to the understanding of this sequence of behavior. The responses that are observed to result from oxygen lack are released as are the other responses. In this typical case, changes in the internal environment are sufficient to allow the release of specific inborn neural patterns, which in turn set off specific adaptive patterns of behavior.

In the fetus and in the prematurely delivered guinea pig, other needs are less easy to demonstrate. Skin cooling, in certain instances, however, does lead to changes in heart rate and breathing rate and even to shivering and motor movement. These responses to thermal stimuli again may be considered as adaptive forms of behavior. They are released by upsetting what has been called the living balance of the organism. The so-called need for food in the prematurely delivered fetus can also be demonstrated. For many days before normal birth it is possible by appropriate stimulation of the fetal lips to bring about typical sucking reflexes, at least so far as the lips and tongue are concerned. This pattern of behavior in the human individual has been called *Thompson's Reflex*. Such responses can be elicited in the fetus long before there is any need for external alimentation on the part of the fetus. It is important to understand, however, that these responses can also be elicited after birth or premature delivery when food is biologically necessary for the survival of the individual. In these latter cases, if liquid is introduced into the mouth, it in turn releases the sucking reactions. The added stimulus of the liquid now sets off the inborn swallowing reflexes, and the ingestion of food from the external world begins. When this course of activity fills the stomach or leads to alterations in the blood stream, the reflex pattern of sucking, as noted above, is altered. External stimuli which a little time before released the sucking reflex now do so no longer. We say that the animal is satiated. A new course of behavior is often elicited by the same

stimulus which previously caused sucking. It may lead to head-turning and so-called avoidance responses.

Similar statements can be made in relation to the inborn or maturationally developed responses related to elimination, rest, change, and sex. Thus, as I see it, the contribution of the experimental embryology of behavior to an understanding of the psychology of need and motive is clear. It suggests that the mechanisms which are released by appropriate internal and external patterns of stimuli in so-called deficit or need states are not set apart from the rest of behavior. They are merely typical of all conditions in which fundamental inborn or learned mechanisms of the organism are brought into play as a result of specific forms of stimulation. The characteristic which has led to the elevation of *need states* as a special form of behavior is, of course, the fact that the stimulation in such cases is often dependent upon changing conditions inside the organism. Behavior so internally determined persists as the organism moves through an external environment made up, it may be, of very varying physical energies. The persistence of behavior associated with such desires, as they are called in traditional psychology, can be understood in terms of the embryology of behavior as a mere extension of the conditions which govern other and simpler types of stimulus-released behavior. This fact is especially important to consider in connection with theories of learning which set up *need-satisfying* forms of response as a special category or even explanation of learning. The facts presented here suggest, rather, that the full internal and external stimulus condition, *before* the successful act rather than after the act, is fundamental in giving a scientific basis or explanation of learning, as recently suggested by Guthrie (27). The view could also be defended that an understanding of the early growth of behavior will make it easier to understand certain other insistent problems of learning (13). The words "natural organization," "spontaneous association," and "pseudoconditioning" describe different forms of behavior of great importance. These phrases take on new meaning when viewed in terms of the gradual growth and inner development of the living response mechanism.

The last paragraphs have given mere hints of fields which seem to me not unrelated to the experimental embryology of mind. They have been introduced to indicate that the description of early individual behavior may eventually be seen to throw light upon some of the basic problems of scientific psychology. In this paper I have attempted to demonstrate that the fetus may be described as machine-like in the precision and repetition of many of its patterns of response in the same stimulus situations. I have also suggested that the complex behavior of the adult organism may in certain aspects be viewed in a similar manner. That is, the machine character of the fetus does not seem to be lost as development progresses, provided only it is recognized in its early stages when it can be most clearly seen. The machine, as well as what it can do, becomes more complex, but it does not become in any sense non-mechanical. This is still true, it seems to me, when the organism's behavior is of the sort that external observers call purposive, or when it is adaptively modified by learning.

Much that I have said in the latter part of this paper has obviously been speculation in advance of full experimental evidence, but I have not made any statements that have not seemed to be forced upon me as the result of my observations of the growth and change in the behavior of organisms before learning begins. I have attempted further to suggest—as little more than an article of belief, it may be—that the experimental embryology of mind is not unrelated to an understanding of adult behavior and adult mental life. Is it not possible that this same point of view will also be useful as we consider social behavior and abnormal human behavior such as that shown in states of extreme anxiety and frustration? Here indeed, as Hughlings Jackson long ago suggested, a knowledge of the evolution of behavior may provide a clue to an understanding of its dissolution.

The scientific psychologist, of course, has other and, it may be, much more basic approaches to an understanding of adult human mental life than that here called the experimental embryology of mind. The philosopher, the esthetician, the moralist, the exponent of religion, and the other students of human values will rightly insist upon the consideration of other and, from certain

points of view, more fundamental approaches to the great aim of the explication of the human mind and spirit. In this paper I shall be more than satisfied if the fetuses and I have done one thing. We shall feel rewarded if what has here been called the experimental embryology of mind can be seen to have some small, but real, contribution to make, even at the simplest level, to a paradoxical and impertinent enterprise—the enterprise which psychologists have taken as their own—the human mind's ambitious attempt to understand itself.

BIBLIOGRAPHY

- 1 ANGULO Y GONZALEZ, A W The prenatal development of behavior in the albino rat *J. comp. Neurol.*, 1932, 55, 395-442
- 2 BARCROFT, J The brain and its environment New Haven Yale Univ Press, 1938
- 3 BARCROFT, J, BARRON, D H, & WINDLE, W F Some observations on genesis of somatic movements in sheep embryos *J. Physiol.*, 1936, 87, 73-78
- 4 BECKER, R F, WINDLE, W F, BARTH, E E, & SCHULZ, M D. Fetal swallowing, gastro-intestinal activity and defecation in amnio an experimental roentgenological study in the guinea pig *Surg Gynec Obstet.*, 1940, 70, 603-614
- 5 BRIDGMAN, C S, & CARMICHAEL, L. An experimental study of the onset of behavior in the fetal guinea pig *J. genet. Psychol.*, 1935, 47, 247-267.
- 6 CARMICHAEL, L Heredity and environment are they antithetical? *J. abnorm. soc. Psychol.*, 1925, 20, 245-260.
- 7 CARMICHAEL, L. The development of behavior in vertebrates experimentally removed from the influence of external stimulation. *Psychol. Rev.*, 1926, 33, 51-58
- 8 CARMICHAEL, L. A further experimental study of the development of behavior. *Psychol. Rev.*, 1928, 35, 253-260.
- 9 CARMICHAEL, L. The experimental study of the development of behavior in vertebrates. *Proc. & Pap., 9th Int. Congr. Psychol.*, New Haven, 1929. Pp. 114-115.
- 10 CARMICHAEL, L. Origin and prenatal growth of behavior. In Murchison, C (Ed.), *A Handbook of Child Psychology*. (2nd rev. ed.) Worcester: Clark Univ. Press, 1933. Pp 31-159.
- 11 CARMICHAEL, L. The genetic development of the kitten's capacity to right itself in the air when falling. *J. genet. Psychol.*, 1934, 44, 453-458
- 12 CARMICHAEL, L. An experimental study in the prenatal guinea pig of the origin and development of reflexes and patterns of behavior in relation to the stimulation of specific receptor areas during the period of active fetal life. *Genet. Psychol. Monogr.*, 1934, 16, 337-491.
- 13 CARMICHAEL, L. A re-evaluation of the concepts of maturation and learning as applied to the early development of behavior. *Psychol. Rev.*, 1936, 43, 450-470.
- 14 CARMICHAEL, L. Stimulus intensity as a determiner of the characteristics of behavior in the fetal guinea pig. (Abstract.) *Science*, 1937, 86, 409.
- 15 CARMICHAEL, L. Fetal behavior and developmental psychology. *Onzième-Congr. int. Psychol. (Rapp. et C. R.)*, Paris, 1938, Pp. 108-123.

- 16 CARMICHAEL, L. A technique for the electrical recording of eye movements in adult and fetal guinea pigs (Title only) *Psychol. Bull.*, 1940, 37, 563
- 17 CARMICHAEL, L., & LEHNER, G. F. J. The development of temperature sensitivity. *J. genet. Psychol.*, 1937, 50, 217-227
- 18 CARMICHAEL, L., & SMITH, M. F. Quantified pressure stimulation and the specificity and generality of response in fetal life. *J. genet. Psychol.*, 1939, 54, 425-434.
- 19 CARPENTER, C. R. Psychobiological studies of social behavior in Aves I. The effect of complete and incomplete gonadectomy on the primary sexual activity of the male pigeon. *J. comp. Psychol.*, 1933, 16, 25-57.
- 20 CARPENTER, C. R. Psychobiological studies of social behavior in Aves II. The effect of complete and incomplete gonadectomy on secondary sexual activity with histological studies. *J. comp. Psychol.*, 1933, 16, 59-97
- 21 CARR, G. Condillac's treatise on the sensations. Los Angeles: Univ Southern California, 1930.
- 22 COGHILL, G. E. Anatomy and the problem of behaviour. Cambridge, Eng. Univ Press; New York. Macmillan, 1929
- 23 COGHILL, G. E. The neuro-embryologic study of behavior: principles, perspective, and aim. *Science*, 1933, 78, 131-138
- 24 COGHILL, G. E. Integration and motivation of behaviour as problems of growth. *J. genet. Psychol.*, 1936, 48, 3-19
- 25 CORONIOS, J. D. The prenatal development of behavior in the cat. *Genet. Psychol. Monogr.*, 1933, 14, 283-386.
- 26 CRAWFORD, M. P. The relation between social dominance and the menstrual cycle in female chimpanzees (Abstract) *Psychol. Bull.*, 1940, 37, 432
- 27 GUTHRIE, E. R. Association and the law of effect. *Psychol. Rev.*, 1940, 47, 127-148
- 28 GUTHRIE, E. R., & HORTON, G. P. A study of the cat in the puzzle-box (Abstract) *Psychol. Bull.*, 1937, 34, 774
- 29 HOOKER, D. Early fetal activity in mammals. *Yale J. Biol. Med.*, 1936, 8, 579-602.
- 30 JASPER, H. H., BRIDGMAN, C. S., & CARMICHAEL, L. An ontogenetic study of cerebral electrical potentials in the guinea pig. *J. exp. Psychol.*, 1937, 21, 63-71.
- 31 KLUVER, H. Behavior mechanisms in monkeys. Chicago: Univ. Chicago Press, 1933.
- 32 KOHLER, W. Gestalt psychology. New York: Liveright, 1929
- 33 KUO, Z. Y. Ontogeny of embryonic behavior in Aves V. The reflex concept in the light of embryonic behavior in birds. *Psychol. Rev.*, 1932, 39, 499-515.
- 34 KUO, Z. Y., & CARMICHAEL, L. A technique for the motion-picture recording of the development of behavior in the chickembryo. *J. Psychol.*, 1937, 4, 343-348.
- 35 KUSSMAUL, A. Untersuchungen über das Seelenleben des neugeborenen Menschen. Leipzig: Winter, 1859.
- 36 LINCOLN, A. W. The behavioral development of the feeding reaction in the white rat. Master's Thesis, Brown Univ., 1933
- 37 MINKOWSKI, M. Zur Entwicklungsgeschichte, Lokalisation und Klinik des Fusssohlenreflexes. *Schweiz. Arch. Neurol. Psychiat.*, 1923, 13, 475-514.
- 38 MINKOWSKI, M. Neurobiologische Studien am menschlichen Foetus. *Handb. biol. ArbMeth.*, 1928, Abt. V, T. 5B, H. 5, 511-618.
- 39 MOWRER, O. H. "Maturation" vs. "learning" in the development of vestibular and optokinetic nystagmus. *J. genet. Psychol.*, 1936, 48, 383-404.

40. MULLER, H. R., & WEED, L. H. Notes on the falling reflex in cats. *Amer J. Physiol.*, 1916, 40, 373-379.
41. ORR, D. W., & WINDLE, W. F. The development of behavior in chick embryos the appearance of somatic movements. *J. comp. Neurol.*, 1934, 60, 271-285.
42. PARKER, G. H. The elementary nervous system. Philadelphia: Lippincott, 1919.
43. PFAFFMANN, C. Differential responses of the new-born cat to gustatory stimuli. *J. genet. Psychol.*, 1936, 49, 61-67.
44. PREYER, W. Die Seele des Kindes. Leipzig: Fernau, 1882. (5th ed., 1900.) The mind of the child. Pt 1 The senses and the will Pt 2. The development of the intellect. (Trans. by H. W. Brown.) New York: Appleton, 1888, 1889.
45. PREYER, W. Spezielle Physiologie des Embryo. Untersuchungen über die Lebenserscheinungen vor der Geburt. Leipzig: Grieben, 1885.
46. RANEY, E. T., & CARMICHAEL, L. Localizing responses to tactual stimuli in the fetal rat in relation to the psychological problem of space perception. *J. genet. Psychol.*, 1934, 45, 3-21.
47. RAWDON-SMITH, A. F., CARMICHAEL, L., & WELLMAN, B. Electrical responses from the cochlea of the fetal guinea pig. *J. exp Psychol.*, 1938, 23, 531-535.
48. RICHTER, C. P., & SCHMIDT, E. C. H. Behavior and anatomical changes produced in rats by pancreatectomy. *Endocrinology*, 1939, 25, 698-706.
49. STONE, C. P. Motivation: drives and incentives. In Moss, F. A., *Comparative Psychology*. New York: Prentice-Hall, 1934. Pp. 73-112.
50. SWENSON, E. A. The development of movement of the albino rat before birth. Doctor's Thesis, Univ. Kansas, 1926.
51. TRACY, H. L. The development of motility and behavior reactions in the toadfish (*Opsanus Tau*). *J. comp. Neurol.*, 1926, 40, 253-360.
52. WARKENTIN, J. A genetic study of vision in animals. Unpublished Doctor's Thesis, Univ. Rochester, 1938.
53. WARKENTIN, J., & CARMICHAEL, L. A study of the development of the air-righting reflex in cats and rabbits. *J. genet. Psychol.*, 1939, 55, 67-80.
54. WARKENTIN, J., & SMITH, K. U. The development of visual acuity in the cat. *J. genet. Psychol.*, 1937, 50, 371-399.
55. WINDLE, W. F. Physiology of the fetus: origin and extent of function in prenatal life. Philadelphia: Saunders, 1940.
56. WINDLE, W. F., & FISH, M. W. The development of the vestibular righting-reflex in the cat. *J. comp. Neurol.*, 1932, 54, 85-96.
57. WINDLE, W. F., & GRIFFIN, A. M. Observations on embryonic and fetal movements of the cat. *J. comp. Neurol.*, 1931, 52, 149-188.
58. WINDLE, W. F., MINEAR, W. L., AUSTIN, M. F., & ORR, D. W. The origin and early development of somatic behavior in the albino rat. *Physiol. Zool.*, 1935, 8, 156-185.
59. WINDLE, W. F., O'DONNELL, J. E., & GLASSHAGLE, E. E. The early development of spontaneous and reflex behavior in cat embryos and fetuses. *Physiol. Zool.*, 1933, 6, 521-541.
60. WINDLE, W. F., ORR, D. W., & MINEAR, W. L. The origin and development of reflexes in the cat during the third fetal week. *Physiol. Zool.*, 1934, 7, 600-617.
61. YANASE, J. Beiträge zur Physiologie der peristaltischen Bewegungen des embryonalen Darmes. I. Mitteilung. *Pflüg. Arch. ges. Physiol.*, 1907, 117, 345-383.

- 62 YANASE, J. Beitrage zur Physiologie der peristaltischen Bewegungen des embryonalen Darmes. II. Mitteilung Beobachtungen an menschlichen Foten *Pflug Arch ges. Physiol.*, 1907, 119, 451-564
- 63 YERKES, R. M eDominanc and sex among chimpanzee (Abstract) *Psychol. Bull*, 1940, 37, 432.
- 64 YOUNG, W. C, DEMPSEY, E. W, & MEYERS, H I Cyclic reproductive behavior in the female guinea pig *J. comp Psychol*, 1935, 19, 313-335.

ANT LEARNING AS A PROBLEM IN COMPARATIVE PSYCHOLOGY

T. C. SCHNEIRLA

American Museum of Natural History
and
New York University

Solomon's well-known counsel to the sluggard stands as a testimonial to the antiquity of man's interest in the activities of ants. Like the weather, these insects are always around, forcing themselves upon our attention in various ways both theoretical and practical. Among the theoretical problems of long standing, wondering how ants reach food and find their way home unquestionably stands near the head of the list. Actually, the question of ant orientation or way-finding was one the first to stimulate serious investigation as a problem in animal behavior. Let us see how this led to the notion that learning has a place in the picture.

The first investigation of ant way-finding was Bonnet's simple finger-test (1779): by rubbing a finger across the route of an ant procession he set up a disturbance which suggested that the travellers were following an actual trail on the ground. Such tests provided the basis for the chemical-trail conception, which held sway in the early literature and now remains as a popular notion that ants in general make their way by following a chemical track.

If all ant orientation were that simple there would be little point in introducing the subject into a serious psychological treatise. However, the phenomenon usually is much more complex

and has a variety of forms, as was made increasingly clear through the 19th century by a series of enthusiastic investigators from Huber (1810) to Forel (1874) and many others.

Although we are mainly interested here in the ant's ability to "straighten her path," as Lubbock (1881) spoke of this insect's learning ability in first calling it to attention, until comparatively recently the main interest of investigators centered on "the senses that guide the ant." If this preoccupation with "sensory control" occasions surprise, let it be remembered that for a considerable time maze investigations of rat learning were focussed largely upon the same problem. Perhaps it is a matter of more than incidental interest to psychologists that the word "learning" itself has been traced to an early root meaning "to follow a track" (Smith, 1912).

Ant orientation proves to be a highly involved phenomenon varying greatly in form among different species. Lubbock was one of the first to demonstrate its great complexity, through work with the garden ant *Lasius niger* showing that this insect establishes its route in dependence upon the direction of light as well as upon the character of chemical traces upon the pathway. On the other hand, Forel's (1908) tests led him to conclude that many species run according to a succession of images, furnished through vision as well as through the "topochemical sense,"—the latter permitting the reception of chemical and tactual stimuli in complex combinations from objects along the path. Later on Cornetz (1910) patiently followed Algerian ants through their wanderings in the open, completed many tracings of routes to and from food-places and performed tests which convinced him that beyond sensory effects the ant must possess an "unknown direction sense." To him it seemed inconceivable that visual and other sensory cues alone could account for results such as the ability of *Myrmecocystus* and *Tapinoma* individuals to make their way fairly directly toward the nest after having been transferred from their route to a "strange" locality. However, Santschi (1911) and others objected that Cornetz's tests were not critical, since his controls and in particular his precautions to exclude visual guidance were not adequate. Around this general question there developed in

the first quarter of the present century a lively controversy involving a number of European investigators, producing a variety of conclusions which were seemingly at cross purposes and generally very confusing. Beneath the apparent chaos, however, a solution to the problem was being worked out. This was finally shown when Rudolf Brun (1914), a Swiss psychiatrist interested in the problem mainly through the influence of Forel, was led to carry out comprehensive field and laboratory tests on the way-finding of a number of species.

Brun's work, based upon the studies of his predecessors which he extended and integrated, brought out the fact that ant orientation is first of all a matter of a patterned sensory control. The sensory pattern, he demonstrated, is different in different species, and may vary in the same species according to conditions. He differentiated between ants such as *Formica* species in which individual foraging is typical and visual effects from large objects cooperate with tactual and other sensory effects, and ants such as *Tapinoma* species in which collective foraging is typical and effects such as light-direction are superadded to a basis of chemical-trail orientation. To account for the formation of these sensory patterns, Brun adopted the "memory-theory" of Semon, concluding that in passing through new terrain the ant "ecphorizes" (*i.e.*, imprints neurally) successive sensory effects which as "engrams" (*i.e.*, memory traces) may be rearoused on further expeditions, guiding the traveller along her route.

Thus contributions to the solution of the orientation problem inevitably had to find some basis in learning-theory. However, Brun's application of Semon's memory-doctrine, like Forel's "succession of images" idea, did not meet the central problem of learning. We find in both of these notions a characteristic evasion of the primary question, to explain how the ant's route changes in the course of successive journeys from a highly involved and circuitous path to a fairly direct one with few detours. How the ant "straightens her path," to use Lubbock's expression, actually embodies the difficult problem of selective learning, which requires us to account not only for the disappearance of certain of the initial activities but also for the gradual develop-

ment of a new arrangement of activities. The Semon theory does not meet the problem of *selection* in learning, since in effect it merely tells us that through experience changes occur in neural function which may influence subsequent behavior when re-aroused. The question to be answered concerns why certain changes are more persistent than others, and why certain ones effectuate in adaptively significant behavioral organizations and others do not.

The fact is that the development of investigations upon ant orientation has given us an impressive amount of information concerning the sensory cues involved, but has not dealt with the genesis of the individual insect's ability to utilize given patterns of sensory cues in way-finding. Most of the European investigators have really taken this learning process for granted, by carrying out their tests with insects able to make their way successfully when first observed. New techniques are necessary if we are to discover how the route is established in the first place.

The maze method has served this purpose, since in the maze we have a representation of the foraging situation under controlled laboratory conditions in which the subject's behavior may be studied in detail from beginning to end. It is an interesting historical fact that the first maze studies on ants were conducted by Fielde (1902) only a few years after the first rats were run in a maze of Hampton Court pattern by Small. Primarily to test the ant's "sense of smell," Fielde used a maze of her own design in which the ants (*Aphaenogaster piceum*) were forced to travel both ways along an open diagonal path from which concentrically arranged detours led off to each side. By marking some of the ants distinctively with water colors, Fielde was able to follow their individual travels. Far more valuable than her findings here on "smell," she discovered that individual ants tended to establish their own routes independently of others, and that in going and coming (carrying their own pupae back to the nest through the maze) the subjects gained speed by cutting down the number of excessive movements. She also reported that the ants "appeared to follow the line of least resistance, or to be influenced by inconvenience"; moreover, that there appeared to be no es-

sential relationship between the outgoing and returning routes of given individuals.—Much confusion might have been avoided if investigators of ant orientation had noticed the significance of this last finding, at a stage when many of them were following the Piéron (1904) dictum that on her return trip the ant simply reverses her outgoing movements.

There followed the studies of Turner (1907) with *Formica subsericea* and other species, from which he reported the first learning (time) curves for ants, as evidence that "the show and exploring gait with which most species make the first few trips of the initial experiment of any series, when contrasted with the rapidity of the later movements, indicates that the ants learn the way home."

As evidence that the "comparative" point of view was influential in studies of animal learning at this stage, despite a general preoccupation with the rat, there are Shepard's (1911; 1914) reports in which results for ant and rat learning tests are discussed together. Using two *Formica* species in a special maze, with precautions to control the effect of chemical traces (by fitting pasteboard lining-units in the alleys) and vision (direction of illumination), Shepard not only studied the importance of sensory cues but also attacked the problem of error-elimination. With the ants free to pass through the maze between their nest and a food-place, studies of successive trips made by particular subjects (marked temporarily, or distinguished through anatomical peculiarities) showed that the ants were capable of learning to avoid blind alleys and also demonstrated that the basis of this change lay in the discrimination of chemical, visual, and other sensory differences at the respective junctions of true path and blind alley. If a lamp remained on one side throughout original learning, shifting it later to the opposite side of the maze produced a substantial disturbance in behavior, indicating the importance of light-direction cues for specific adjustments in the route. Or after all pathway linings had remained in place through a series of runs, exchanging the lining units of the true-pathway and blind-alley alternatives at a given junction accounted for serious difficulties in the vicinity, with a noticeable tendency

to turn into the blind alley. The subjects were also disturbed by changes in the pattern of the maze (such as blocking off a previous true-pathway unit and introducing a new one), although they were able to readjust in the course of further runs. The ant's maze-learning thus seemed to be a matter of considerable complexity, which Shepard was able to describe as a trial-and-error process resembling that of the rat although of a much simpler order.

Such work does more than merely to demonstrate that another animal is capable of learning mazes; beyond improving our conceptions of insect psychology it has relevance to general problems of broad psychological interest. Specifically, insect maze work affords us a valuable way of studying the learning process under organic and psychological conditions which appear to be significantly different from those in mammalian behavior.

Our main object in this chapter is to explore the possibilities of studying insect problem-solving as a distinctive attack upon the learning phenomenon. What important similarities and what differences will be found in the ant, which completely lacks cereb cortex, and a representative lower mammal which possesses cortex? At first sight the similarities appear to be more important than the differences. That is an impression usually given initially by the fact that not only are some ants capable of mastering mazes which prove to be fairly difficult for naive rats (Fig. 1), but also that similar learning curves of the "negatively accelerated" type are obtained (Fig. 3). It is only on closer examination that some prevalent differences in performance become apparent which lead us to view the ant's maze-learning process as a qualitatively distinctive instance of complex learning. In view of this fact it is apparent that this instance of learning must be dealt with first of all in its own terms before comparisons with other animal performances can have much validity.

As we have pointed out, a chief advantage of the maze situation rests in the fact that it permits us to present under controlled conditions certain obstructions which are essentially equivalent to difficulties encountered in the natural foraging situation. To improve experimental control, in the writer's recent experiments

(1941; 1943) the maze has been presented as an obstacle to the ant's passage from nest to food-place, or from food-place to nest, but the subject ordinarily is prevented from returning through

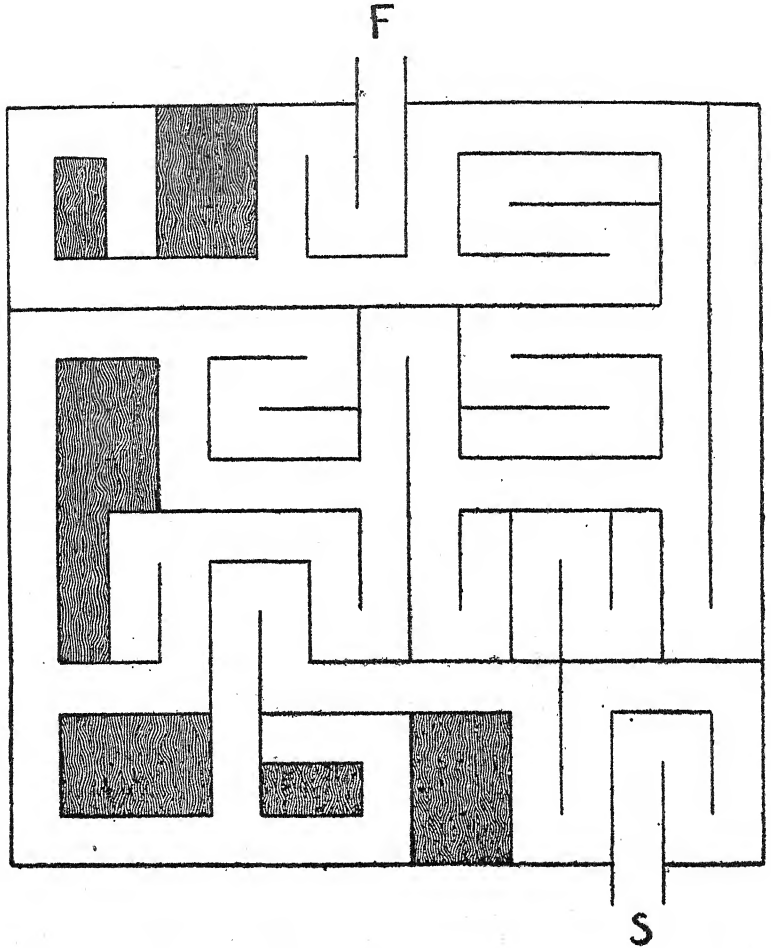


Figure 1.

Maze pattern designed for rats; also mastered by ants. (S, start; F, food-box.)

the pathway used in reaching the food-place. In the double maze shown in Fig. 2, maze pattern *D* may be used under either or both of two general conditions in the foraging run (*i.e.*, *R*, nest to food-place; *L*, food-place to nest), with simple paths around

the maze for phases of the run in which the maze is not used. As a regular problem, however presented, the maze must be run through from alley 1 to alley 13e.

In the experiments we shall discuss, steps were taken to confine effective sensory cues in learning to the intra-maze situation

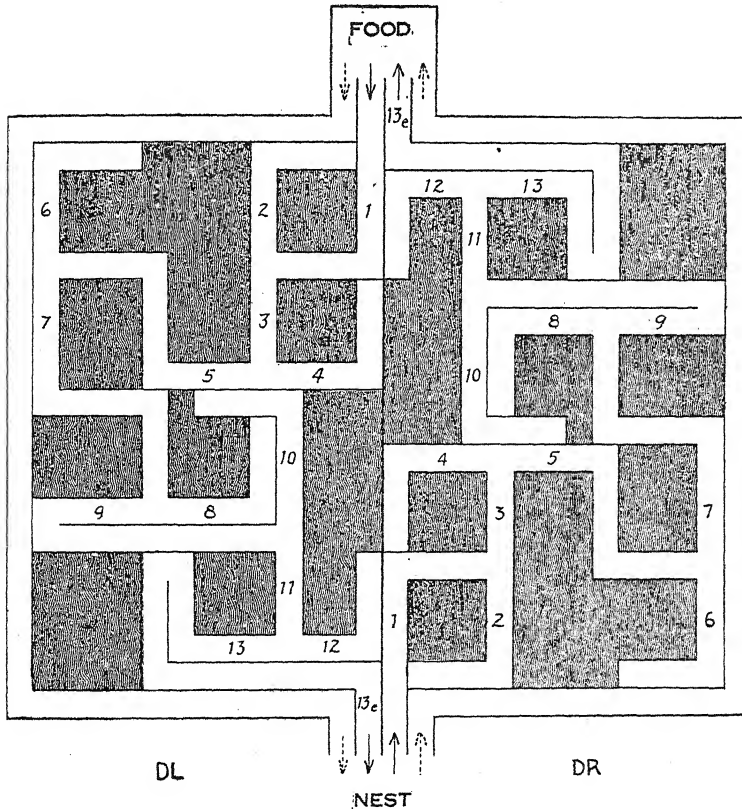


Figure 2.

Maze pattern *D*, used as a problem opposing passage from nest to food-place (situation *DR*) or from food-place to nest (situation *DL*). By using the simple pathway around the maze, either of these situations may be presented as a single problem.

itself. The main reason was that when extra-maze cues such as directionalized light are available, the problem is simplified for the subject in a way that greatly increases the experimenter's problem of understanding the learning process from the per-

formance records. For this reason, diffuse illumination was used, the light non-directionalized and effectively equal in intensity for all sections of the maze. With other precautions such as a hood screening off the observer (who watched events through any one of four small apertures symmetrically arranged around the sides of the hood), this served to confine effective sensory cues to the maze alleys themselves. All alleys of the maze were lined with bristol-board pathway units covering floor and walls, and the maze was covered with glass, as in Shepard's situation, to permit a control of chemical and related cues within the maze.

With these arrangements in the main experiment, the maze was presented under very different conditions to two principal groups of subjects. One group, (A) the normal subjects, was permitted to learn the maze with the pathway linings undisturbed throughout; the other group, C, was subjected to an unstable intra-maze situation by virtue of numerous exchanges in the position of pathway linings throughout the course of learning. (Or, in other cases in group C, a different set of pathway linings was inserted after every trial.) The object of this method was to set up a critical difference in the availability of intra-maze cues which might cast light upon the essential process of learning.

In all of this work the species used was *Formica incerta*, an individual-foraging ant of the subfamily *Camponotinae* which adapts very well to laboratory conditions and appears to represent the maximum performance level of ants in this type of problem. Only one ant was admitted to the maze in a given session, and others were excluded by means of a system of entrance and exit doors which also permitted controlling the movements of the given subject.* The selected subject was admitted for as many

* The colony is prepared for work at a given time of day by feeding it every day at this time through the preceding two weeks. The learned ability to forage at a special time, known as "temporal memory" (Grabensberger, 1933), insures that the subject will be available for work at that time. Unfortunately, other members of the colony, similarly trained, are also especially active and impede operation of the apparatus by gathering near the maze doors. The difficulty recurs each time the return of the record ant rearouses ants in the nest. However, if the others are not admitted to the maze, their appearance from the nest is inhibited in time through learning, so that after an hour or two the maze subject may be the only individual seen outside the nest.

runs as she would make in a given series,—*i.e.*, learning by the "massed repetitions" method, which seems to be typical of foraging in the natural situation. The ants were distinguished from one another by means of readily identified symbols on enamelled tissue attached to the dorsal surface of the gaster with rubber

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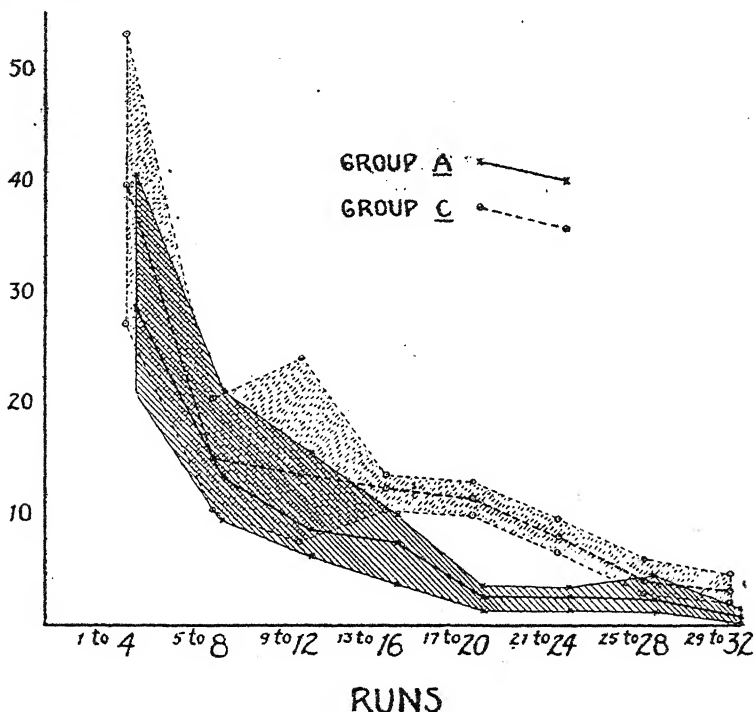


Figure 3.

The general-average error curves for group *A* (normal) and group *C* (experimental) in situation *DL*. The differently shaded zones represent the intervals between the maximal and minimal performances in the respective groups (4 ants each group).

cement. Shorthand records were kept of the subject's movements, in particular the nature and locality of maze activities classed as "errors" (*i.e.*, retracing; entrances into blind alleys); the records also included feeding time, maze time, and time in nest between runs.

For reasons to be considered later, these ants perform at their best when running through the maze from food-place to nest (Schneirla, 1933), carrying small bits of raw beef. Groups *A* (stable maze situation) and *C* (unstable intra-maze cues) were both tested under that condition, hence any differences in their results may be attributed to the single difference between them, affecting conditions within the maze. Although negatively accelerated error curves are typical in both groups, as Fig. 3 suggests, subjects in the experimental group *C* required more runs to master the maze than normal subjects (*A*) and exhibited special difficulties beyond those of normals. A comparison of the results for these groups proves to be a most useful way of disclosing how this problem situation is learned by ants.

Through such a comparison we find that three principal stages may be identified in the course of the ant's maze-learning: in succession, (1) an initial stage of general maze adjustment (Schneirla, 1941), (2) an intermediate stage of specific localized learning (Schneirla, 1943), and (3) a terminal stage of inter-segmental organization. Let us briefly survey the evidence for this description of the habit.

In their early runs the ants of both groups behave similarly and accomplish similar improvements, dropping errors such as running on alley ceilings, returning repeatedly to the starting point, circling on walls and in corners, and turning back frequently from points in open alleyway. Such changes are responsible for the initial abrupt drop which characterizes the individual error curves (Fig. 4), a change which typically runs its course within the first 8 or 10 runs. This series of trials brings generalized improvements in behavior not bound to any particular locality in the maze, representing a broad adjustment to the situation which may be roughly characterized as "maze-habitation." In these early runs the ant becomes able to run smoothly through maze alleys, without turning back unless an obstruction (*i.e.*, a corner or end-wall) is encountered. Can we say that a "route" (*i.e.*, a particular path to the end-point of the maze) has been learned?

A study of the errors which drop out during these early

runs shows that they are not particular or localized activities but rather are random, variable movements which produce retracing or incidentally arise through retracing anywhere in the maze. This learned change is apparent even on the first run, when after much variable activity and back-tracking in the first parts of the situation the subject runs more smoothly and passes through the latter part of the maze with definitely fewer errors, although these alleys are distinctive in pattern and she has not passed through them previously. The benefits of initial-habituating learning thus may be elicited in any part of the situation regardless of its pattern, for these changes do not depend upon the specific local patterning of the maze. The critical fact is that through this initial stage there is no identifiable shortening of entrances into blind alleys anywhere in the maze, nor is there any change in the proportion of blind-alley turns to true-pathway turns at the choice-points which would indicate a developing ability to avoid blind alleys. Blind-alley entrances are reduced in frequency, but simply as an outcome of a reduction of retracing generally. It appears that under our conditions the subjects do not learn a particular route during the initial stage, but are essentially restricted to a habituation-learning.

A comparison of groups *A* and *C* is enlightening as to the distinction between the first two stages of learning. In their early runs subjects in group *C* seem to be much more disturbed than *A* subjects and make more errors, yet these are the same kinds of errors and they are dropped similarly in the two groups. The *C* subjects spend more time in the maze and have more difficulty than *A* subjects, yet toward the end of the first ten runs we find that the two groups have reached a comparable level of improved performance (see Fig. 3). This suggests that the learning which has been accomplished is generalized in nature, and not dependent upon particular local cues, since it occurs in group *C* despite an inconstancy of local stimulation. What is acquired seems to be a non-specific "alley-running set," a generalized way of behaving in the situation rather than specialized adjustments to stimulus cues as fixed patterns or local landmarks. However, as later events show, these early developments open the way for a specific

mastery of the problem, and are really essential if such specialized learning is to occur.

To further progress in learning the maze the initial stage contributes a smooth-running behavior in the situation, insuring in particular a routine manner of approaching choice-points and entering blind alleys. This may be described as a yielding of the habituated subject to movement-mechanics effects dependent upon the pattern of local alley-sequences, a rudimentary type of "set" which we have termed "centrifugal swing" (Schneirla, 1929). The stereotypy in locomotion thereby accomplished materially influences the further course of learning, determining especially the relative difficulty experienced by the subject in eliminating different blind alleys. At this point, when stereotypy is critically essential for the accomplishment of new learning, whatever introduces undue variability in behavior may seriously retard or even block further progress. The fact we have mentioned, that maze *D* was more difficult when presented in the run to food-box than in the run to the nest (Schneirla, 1933), may be attributed to a large extent to the greater variability of behavior which characterized the ant's run from the nest. Further events in group *C* to be described show that even in the optimal situation, when running with food to the nest, reducing stereotypy at this juncture impairs the new stage of learning.

The intermediate stage, which typically begins in situation *DL* (see Fig. 2) at about the 8th to 10th runs, is identifiable in normal subjects in terms of the representative phases of blind-elimination, which are: (1) the dropping of the second arm of the blind essentially as a unit; (2) full-length entrances to first arms gradually are displaced by very short entrances; and finally (3) the short first-arm entrances fall off in favor of direct true-path entrances. Although this process advances at different rates according to the difficulty of the respective blinds (*i.e.*, how strongly centrifugal swing forces turning into the given blind), when studied comparatively for all sections of the maze the regular order of its successive phases in normal subjects reveals a similar process of specific learning at all difficult choice-points.

These processes have been called *specific* because the man-

ner in which they begin and run their course shows that they occur independently in the respective choice-point localities. At basis this localized learning may be understood as a matter of discrete conditioned-response processes, with the initiating factor in each case the blocking of progress at the dead-end of the given blind (Schneirla, 1943). We may regard this last experience as the unconditioned stimulus for the inhibition of forward progress, for which stimulus patterns experienced earlier in the given blind become equivalent successively in the order of their temporal and spatial relations. These originally effective experiences may be considered the "new" or "conditioned" stimulus patterns which successively are added to the stimulus-complex adequate to elicit avoidance of the given blind-alley situation. From the manner in which blind-avoidance characteristically improves, we may say that first the subject turns around only when actually blocked, next in response to stimulation close to the dead-end itself, then still earlier in the blind in response to the pattern effective at the elbow, and finally the stimulus-pattern encountered at the choice-point becomes adequate to call out avoidance of the blind as a whole. Basically, then, we view the process of blind-dropping which follows the initial stage as a set of conditioned-response adjustments originating independently in each section of the maze where detour-blocking occurs.

The local adjustment described develops as a unitary process which under normal conditions (*i.e.*, the situation of group *A*) expands regularly to become controlled by prominent stimulus-patterns successively more remote from the nuclear unconditioned effect. However, as the results for group *C* (Fig. 3) show, when irregularity and variation impair the sequence of events in the blind-situation from trip to trip, the elimination process develops abnormally and as a rule incompletely. This we have suggested above, in stressing the importance of behavior-stereotypy as a prerequisite for intermediate-stage learning.

This theoretical description of the local adjustments is not completed by the postulation of nuclear conditioning processes alone. Our findings necessitate the further postulation that as each segmental-learning process advances it becomes qualitatively

modified into a selective-learning process, based upon the differential effectiveness of alternative responses near the choice-point. To express the idea briefly, we may view the situation at a fairly advanced stage, when upon barely turning into the blind (generally less than one inch around the choice-point corner) the ant sometimes quickly turns to the side of the true pathway, but at other times hesitates tensely for a brief interval and then proceeds deeper into the blind. This act of pausing on the threshold, as it were, suggests that a focal stimulus-effect encountered there has aroused a more or less complete inhibition of further progress into the blind. That the inhibitory agent is an active process of "braking" forward locomotion is indicated not only by the tenseness of posture during the pause, but also by the forward spring (onward into the blind) or rapid volte face (into true pathway) which follows it, as though released from restraint. The signs of postural tension are even more evident when the subject happens to reach the elbow ending the first arm, frequently taking the form of successive short advances beyond the corner before the ant can leave the blind. We may say that at this juncture the aroused tension-response is resolved into free movement most promptly after a short entrance into the first arm, much less promptly and effectively at the first elbow,—and that this difference gives these adjustments a correspondingly different facilitation in the selective process. Gradually, at different rates in the different blind localities, this selection process advances so that the initial centrifugal-swing-enforced blindward turn is dominated more and more effectively by the stimulus pattern of the true-pathway turn. This last response, when the blind-avoidance process has advanced sufficiently, is very effectively facilitated by the unrestrained movement which follows a briefly aroused tension-effect at the choice-point.

To summarize, we may say that each blind is eliminated by an expansion of the crude blind-end reaction through successive phases involving the addition of more remote stimulus-foci to the system, leading into a selective competition of the respective differently facilitated adjustments. Through this process there develops an "anticipation" of the blind-end in the form of a *set*

away from its side; then finally when this tension-response can be touched off in the immediate vicinity of the choice-point, it may function as a factor added to the facilitation of the direct true-path turn.

These processes of segmental learning constitute the major events of the intermediate stage, accounting for a gradual fall in the error curve when they are effectuated, or for a plateau in the curve when they are retarded (*e.g.*, as in group *C*, Fig 3) or more fully arrested (*e.g.*, as in the case of ant *K*, Fig. 4). Understanding the nature of this process is greatly assisted by an examination of plateau phenomena like those evidenced in group *C*, laboring under inconstant intra-maze conditions. After having accomplished the initial-stage habituation, the subjects of this group began to eliminate entrances into blinds, but virtually from the start this occurred much more slowly and variably than in group *A* subjects. The only phase of blind-elimination that was effectively completed was the first, the dropping of second-arm entrances; later stages leading to complete elimination progressed erratically, and seldom ran their entire course in any choice-point locality. Not only was the blind-shortening process considerably *retarded* in group *C*, but there was a marked *delay* in reaching the point at which there began a regular increase in direct true-path turns at choice-points. In group *A* the frequency of direct true-path turns rose from 49% at trip 8 to 96% at trip 32; however, in group *C* this value remained near 50% until trip 20, and reached its maximum of 85% only at trip 40. The upshot was that although the complete mastery of all six choice-points was accomplished by normal subjects within 32 runs, at the 45th run not one group-*C* subject had mastered all of the blinds. The curves in Fig. 4 express clearly the difference which existed in the performances of these two groups throughout the intermediate stage.

The handicap of group *C* may be attributed to the marked variability which appeared in their behavior following the general maze adjustment of the initial stage. We may say that the disturbance of the tentatively established movement-stereotypy at this critical point disordered the manner in which the focal

stimulus-patterns were encountered just before blocking occurred in the given blind, thereby interfering with expansion of the nuclear blind-conditioning process in the segment. This disarrangement of the temporal sequence of events from trip to trip greatly complicated the selective-learning process; the typical be-

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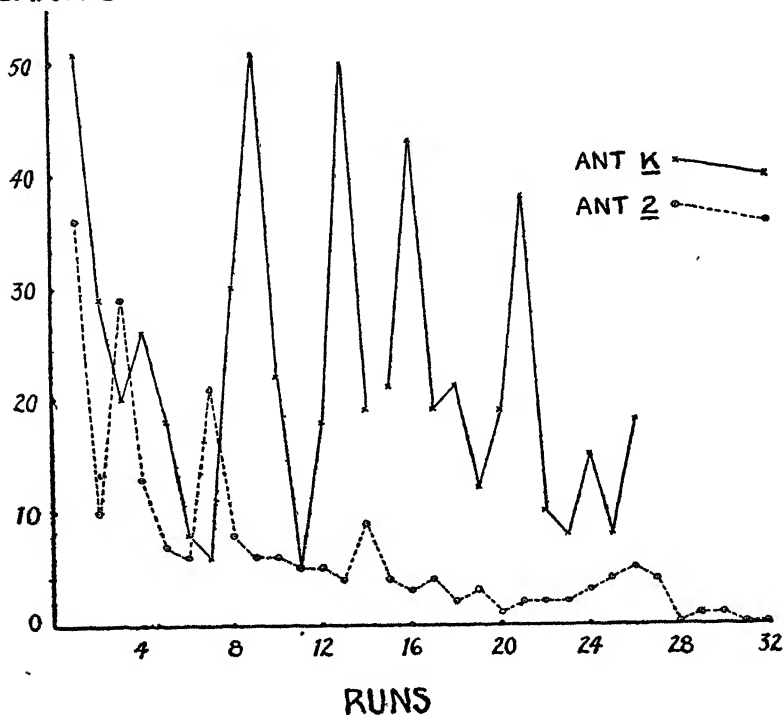


Figure 4.

Error curves for two *F. incerta* subjects learning situation DL. The record of the superior subject (No. 2) was made in a consecutive series of runs, that of the inferior subject (K) on two different days, with an interval of three days following the 14th run.

havior difficulties of these subjects indicating clearly that too many interfering adjustments developed in each locality to permit a consistent, regular advance in blind-shortening. Furthermore, the marked delay in the mastery of choice-point adjustments by group C shows that under normal conditions such learning de-

depends upon reaching a fairly advanced stage in the blind-shortening process. Even under conditions of a constant maze environment, subjects like ant *K* (Fig. 4) which are inferior in stereotyping their initial maze adjustment, cannot progress far in the segmental learning process.

This dependence upon a regularity of stimulus events in the problem situation, and the necessity of stereotyping early behavior if specialized learning is to occur effectively suggests a constitutional limitation of the terrestrial insect for trial-and-error learning. An inconstant maze situation of the type presented to group *C* apparently demands wider resources for selective learning than the ant possesses. To be sure, this insect can master limited alterations in a learned situation (such as a change in the principal direction of illumination,—Schneirla, 1929) provided that such changes may be encountered successively in the same form. Judging from comparable experiments with the rat in which environmental changes have been introduced more complexly (*e.g.*, Honzik, 1936), lower mammals have wider resources than social insects for learned adjustment to a shifting environment.

We have traced the ant's maze adjustment to the point at which local difficulties have been overcome through independent learning processes. Ordinarily, after the initial stage, there is no particular improvement in the running of true-pathway until the segmental learning has reached an advanced stage. Then, in what may be called the *terminal stage*, a unitary maze adjustment is established on a new basis. In effect, when the local choice-point adjustments have extended spatially so that they influence behavior at increasing distances from the critical junctions, they begin to interfere with one another in adjoining sections of the maze, forcing new adjustments in the running of true pathway where they overlap. In normal subjects, the development of a new way of leaving the given choice-point presents the possibility of an interference with the established mode of approaching the next choice-point. Proceedings are further complicated by the fact that approach to a given choice-point typically changes as an extension of the local learning process, building

up literally backward from the junction. As a consequence, new difficulties usually develop in the running of true pathway once the blinds are fairly well mastered. Then, frequently after passing an alley or two beyond a choice-point, the subject will hesitate or stop abruptly, perhaps retracing one or more times in the vicinity, unable to adjust to the situation after entering it somewhat differently than before.

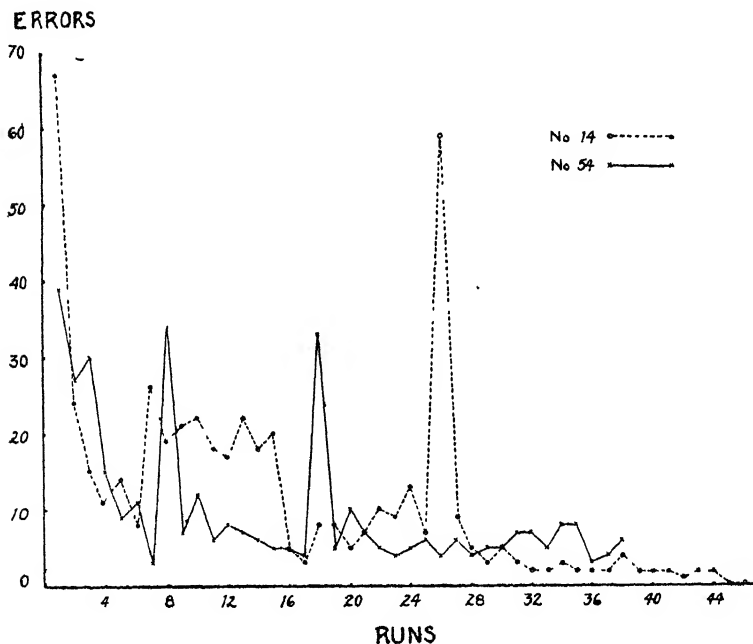


Figure 5.

Two *F. incerta* learning curves from situation DL, illustrating special difficulties in the intermediate period which may (as in No. 14) or may not (as in No. 54) lead directly to a general improvement of the maze response. See text.

Although such reciprocal interferences between learned segments are a typical source of new errors in the terminal stage, through behavior difficulties thereby arising new true-pathway adjustments may become established which link the segmental adjustments into a truly unitary maze response. In Fig. 5 the two error curves, one (case No. 14) representing the eventual completion of this terminal-stage process, the other (No. 54) an

inability to readjust to the new difficulties, with intersegmental errors persisting to the end. The case of subject No. 14 is somewhat extreme, in that difficulties mounted simultaneously in two areas of the maze,—between choice-points 4, 5 and 6, 7 and between 6, 7 and 8, 9. The disorientation in the two true-pathway sequences rose to a high peak on trip 26, however, in both zones new readjustments were promptly accomplished, so that after trip 30 the maze run was completed consistently at a level of efficiency previously unobserved. Although the completion of the terminal stage usually occurs more gradually than with this subject, the case typifies the manner in which an interaction of intermediate-stage adjustments may lead to an improved organization of the maze response.

To sum up, this interpretation of the ant's maze learning holds that a generalized maze habituation first occurs, providing a basis in stereotyped behavior for the subsequent learning of segmental adjustments, and that finally a unitary organized maze behavior may arise through the interaction of these "islands" of learning once they have reached a fairly advanced stage of development.

This process appears to have its closest parallel in the stereotyped procedure of typical rote learning in which a problem is mastered in terms of the literal order of its component segments, describable as a rudimentary type of performance in mammalian subjects. The rote-like manner in which ants characteristically master these maze problems resembles rather closely the stereotyped progress of rats in learning mazes after serious cortical loss.

From this view, we should expect to find outstanding qualitative differences when the ant's characteristic maze adjustment is compared with that of normal mammalian subjects. As far as comparisons can be made at the present time, they tend to confirm that expectation. When normal rats are tested in maze-pattern *D* under conditions broadly equivalent to those holding for ants in group *A*, they master the problem within relatively few runs (all of 10 subjects within 12 runs), in a facile manner quite different from the rather labored and stereotyped process of distinctive stages found in ants. From the rat's first run there are

indications of intersegmental relationships in the situation, together with rapid and complex true-pathway readjustments and the elimination of some of the blinds virtually as wholes. With ants, pattern *D* is useful as a means of studying the learning process; however, rats master this problem with such facility that only very general statements can be made concerning the process of their learning. A comparison of the general error

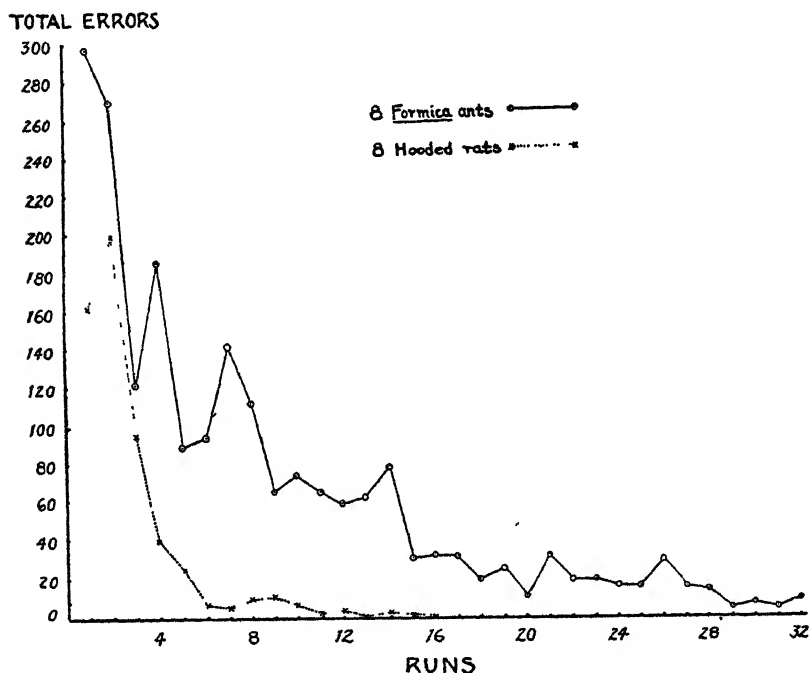


Figure 6.

Cumulative error curves for groups of ants and rats learning pattern *D*.

curves in Fig. 6 will serve to emphasize the marked difference between the performance of ants and rats.

The effect of reversing the maze following original learning clearly reveals the inferiority of the ant's adjustment. When ants are introduced into the last alley after having learned the maze *D*, so that now they must run through it from alley 13e to alley 1, they seem very disoriented and learn this problem (if at all)

only in the course of a long series of runs. On the other hand, after having learned pattern *D* rats master the reversed problem quite readily, obviously profiting by transfer effects from the original learning. It is apparent that these animals must have learned the original problem in very different ways, since only the rat can utilize its learning effectively under the changed conditions.

The ant's limitations are discernible in original learning, in the fact that the segments of the problem primarily are learned strictly in terms of local adjustments. This restriction of the terms of learning extends even to the fact that forward-going blind-alley elimination, a distinct and readily recognizable process as we have seen, is not assisted or influenced in any identifiable way by the experience of running into the blinds when retracing. During the development of blind-elimination, the shortening of entrances becomes increasingly apparent in forward approaches to the choice-points, yet at the same time there is no dependable sign of blind-shortening when blinds are entered during back-tracking. Such entrances into blinds decrease in strict dependence upon the reduction of retracing, and finally disappear only when retracing itself drops out, clearly in a very different manner than we have described for forward-going entrances. In this maze pattern the choice-points were all T's in the forward direction and L's in back-tracking (Fig. 2), affording a marked dissimilarity in the approaches to blinds from the two directions. For the ant, this evidently excluded any common effects in learning,—*i.e.*, from the two directions of approach any given blind apparently was psychologically a very different situation. With this handicap the ants learned nothing specific about the maze when retracing which could assist them later in running the reversed maze; furthermore, any effective transfer from the original forward-going accomplishments was excluded on the same basis. In decided contrast to this state of affairs, rats in the original learning of pattern *D* were able to shorten blind-alley entrances both in forward-going and in retracing, and also exhibited a definite "saving" when introduced to the reversed pattern. Improving greatly on the performance of ants, the rats in their original

learning were able to establish a somewhat common basis of response to any difficult segment of the problem however approached.

Numerous experimenters (*e.g.*, Spence, 1932; Spragg, 1934) have reported evidence for intra-maze transfer in the rat, indicating the existence of integrative learning even during early trials. A notable fact is that the most rapid progress in avoiding blinds appears at the last choice-point and at other distinctive choice-points, demonstrably influencing adjustments at other critical junctions on the basis of pattern similarities such as the direction of turn. Specific transfer effects comparable to these do not appear in the ant's learning, in which the local segments of the problem evidently are mastered independently. For example, when a special group of ants was introduced to situation *DL* with just one blind (at choice-point 8, 9), they eliminated this blind alley much as did subjects that had the full number of six blinds. In other words, the fact that in the complete pattern two other blinds (No. 4, and the terminal blind, No. 12) turned *left* as did the comparison blind (No. 8) did not influence the subject's mastery of the latter situation. This finding indicates that in learning a given segment of the problem the ant is held to the specific terms of the local stimulus situation, disclosing as do the above results a distinctly inferior capacity for problem-organization as compared with the rat.

The ant's proneness for highly stereotyped and situation-fixed learning has been illustrated by her inability to show any effective transfer of learning when a mastered maze pattern is reversed. This characteristic shows itself even more strikingly in the learning of maze segments which (to the experimenter) are identical in pattern, when these are presented simultaneously in different situations. For example, one group of *F. incerta* subjects was presented with maze *D* twice on each trip, running through facsimile patterns in reaching the food-place and in returning to the nest (Fig. 2). After this difficult round trip had been learned (Schneirla, 1934), the entrance problem was modified by blocking alley 9a and opening its previously "blind" alternative alley 8 as a true pathway. The exit problem (*DL*) was

not changed, *i.e.*, in this run alley 8 remained a blind, alley 9 true pathway. Following the change, in their *entrance* runs the ants were at first greatly disturbed at choice-point 8-9, persistently repeating the learned entrance into the now-blocked alley 9, and back-tracking after very short entrances into alley 8, as characteristic errors. In the course of less than twelve runs, however, all three of the tested subjects eliminated the tendency to enter alley 9 and consistently turned directly into alley 8. The interesting fact was that throughout the series of readjustive *DR* runs, on the *return* trip through the facsimile *D* pattern (in *DL*) there were no signs of disturbance and no false moves in passing choice-point 8-9. In that situation, shortly after having had great difficulty in passing through the corresponding part of situation *DR*, on every return run through *DL*, the subjects continued to make the direct turn into alley 9. In this way the ants demonstrated in their behavior that for them "pattern *D*" was a distinctly and rather completely different problem on the two phases of each round trip.

With the normal rat as subject, we should expect to find evidences of a largely common process of learning when a given maze pattern is encountered successively in different situations, resulting in disturbances in all situations when post-learning changes are made locally in one situation alone. In other words, in the original learning process of the rat each run through the pattern in any one of the situations would influence the common habitual adjustment. In this type of test the characteristic stereotypy of the ant's procedure in learning constitutes an advantage so far as restricting the disturbing effects of local change is concerned, yet in the last analysis this is a doubtful advantage since it depends upon a constitutional inferiority in the insect mode of learning.

These results serve to cast doubt upon the validity of the Piéron (1904) hypothesis, widely accepted by writers upon insect orientation. This hypothesis holds that in normal foraging "the return is a function of the outgoing trip" in the sense that the ant is able to repeat its outgoing journey in reverse when returning to the nest. Piéron's judgment, the basis of this ability

must lie in a "muscular memory." In opposition to this notion that a close functional relationship exists between the two phases of the foraging trip, our findings show that the outward and return runs are distinctively different experiences to which the insect learns separate and independent adjustments. The Piéron hypothesis calls for important transfer effects in mastering the two phases of the run, yet in our investigations no evidences of such relationships have appeared. The two phases of the run seem to be mastered as essentially independent problems, whether or not the ant returns through the "same" maze alleys (Schneirla, 1929) or through a facsimile pattern (*i.e.*, through the alleys in the literal order of entrance). Psychologically, the ant's habit seems to be bound by the specific conditions of the given situation in which it is learned, a feature which reminds us of the characteristic limitations of recall in human rote learning.

This interpretation of our subject's limitations is significant beyond the maze-learning of the ant, judging by the findings of other experimenters (Hannes, 1930; Ojfinger, 1931) with other insects in very different situations. Opfinger presented the honey-bee with a distinctive visual environment during each principal phase of the foraging trip, exposing a *blue* patch at the food-place during the *arrival* flight, a *white* patch beneath the food-place during *feeding*, and a *yellow* patch during the *departure* flight. Then, in post-training tests, all of these stimulus-patches were laid out at random on the feeding table, together with various new stimuli. Characteristically, as long as the *blue* patch was present the bees alighted upon it in greatest numbers upon their *arrival* from the hive, but when this patch was removed the bees responded on an unlearned basis, behaving as though the "feeding stimulus" (the white) and the "departure stimulus" were quite as new as the others. These results disclose the interesting fact that for the specific act of arrival there was only one adequate stimulus in the test,—the (blue) one experienced during *arrival* in the training period. Although the other two stimuli present in the preparatory period may have been effective in connection with their respective situations, they lacked any demonstrable influence upon the arrival flight. This is evidence for a psycho-

logical independence among the three phases of the foraging act, each more or less isolated in terms of a specific control by its distinctive stimulus setting. These results from another insect as subject and a different problem situation corroborate our maze findings in suggesting that stereotypy and situation-specific recall are prevalent characteristics of insect learning.

It is evident from this description that the process of problem-solving in insects differs qualitatively in rather fundamental ways from that of the rat. There are similarities, of course, in the general trend of the error curves and in the fact that the *Formica* ant can learn mazes which prove to be fairly difficult for naive rats. However, closer examination reveals differences which suggest a fundamental disparity in the learning processes of insect and mammal. What initially is called the "same" problem is learned very differently by the two animals. Thus we learn that the contour of a learning curve alone is a feature of questionable significance until the underlying organization of the performance is known. The point is vividly illustrated by the fact that some of our error curves (*e.g.*, Fig. 5) exhibit sudden drops in their latter stages which strikingly resemble mammalian curve patterns often taken to indicate the presence of "insight learning." * The basis of the sudden change is a reorganization of the habit, it is true; however, this reorganization in the ant proves to be a process scarcely answering the criteria of human "insight." Far from insight, the ant accomplishes its readjustment in the problem through a relatively crude interaction of serial reactions which have developed independently in adjoining sectors of the problem. In the study of insect behavior, surprising things may be observed which superficially suggest a high

Cf. Maier and Schneirla (1935), Chapt XX.

* From this point of view a long series of impressive performances by insects is described very entertainingly by Hingston in his book, "Instinct and Intelligence," the deductive interpretive procedure of which is suggested by the title. Concerning such procedures, Remy de Gourmont said: "The question of instinct is perhaps the most enervating of all. The simple-minded think it is resolved when they oppose against it the other word: intelligence. That is the elementary statement of the problem, and nothing more. Not only does it not explain anything, but it is opposed to every explanation." (1932, p. 157)

level of psychological process,* but which on closer analysis prove to be much simpler.

More than once in the literature it has been suggested that a common qualitative process is involved in all maze learning, if not in all learning. A recent notable example is the unique treatise of Bierens de Haan (1937), in which a survey of the entire field of animal maze-learning centers around the following proposition.

"Detour and maze are only different in degree; running the maze can be regarded as the making of a complicated detour. Both are based on the grasping, directly present or gradually developed, of spatial relations with regard to the goal . . . in both cases the animal strives to reach a goal, tries to do this by the shortest detour, now that the straight way is closed, makes use of different sensations, forms ideas about the way to the goal, etc. In learning the way in a maze the animal acquires a knowledge of the maze and the spatial relations of its parts, in principle not different from that of a man who learns to find the way home in a foreign town." (p. 212)

Evidence on maze-learning in inframammalian animals is reported in early sections of the monograph, and from this Bierens de Haan seems to conclude circumstantially that these cases fall within his goal-orientation interpretation later offered for the rat. Yet as we have pointed out in a specific comparison, a strong case can be made for the thesis that basic and essential qualitative differences exist between the characteristic learning processes of insect and mammal.

It is not our problem here to discuss the validity of goal-orientation concepts as generalized attempts to explain mammalian learning. It is in order, however, to say that the adequacy of such a conceptualization of learning has not been established by any means, notwithstanding conclusions such as White's (1943) that the "principles of perceptual learning and path-goal behavior . . . ought to be the explicit and universally accepted foundation of all learning theory." (p. 185) In this connection it is the application of this doctrine to insect learning to which we must take exception. Unfortunately, it is not an easy matter to discuss "goal" concepts of animal learning, since their implications typically are as vague and elusive as they are numerous.

To justify the introduction of "path-goal" concepts, however, it is reasonable to ask for a demonstration that the subject's learning process is influenced in identifiable ways by anticipation of a goal (*i.e.*, by response in advance to a spatially remote incentive). We have not found such evidence for the ant.

A deductive application of Gestalt conceptions to the maze situation has led Snygg (1936) to describe the rat's maze learning as a process of "perception," in which blind-alley adjustments are individuated as special patterns from a generalized goal-awareness established during the first few runs. While the relevance of this doctrine for the rat is still a moot question, it is scarcely supported by the ant's maze performance (Schneirla, 1943, pp. 171 ff.). To be sure, at first sight the occurrence of an initial stage of generalized learning followed by a stage of specialized local adjustments might appear consistent with such a view. However, in the initial stage a general habituation to the maze situation is learned, rather than a general start-finish orientation. This habituation permits the subject to run smoothly in maze alleys, until physically obstructed; thus her behavior may be described as a yielding to the effect of pattern-sequences upon movement mechanics, without any qualitative relationship to the location of the end-point. There is no identifiable set toward a "goal,"—on the other hand, the initial stage contributes a movement-stereotypy which in the ant is essential if specific adjustments are to develop in the problem.

Although much human learning is influenced by the anticipation of goals, insect learning seems to be restricted within the bounds of a much simpler motivation. For the insect a more adequate concept is "drive," denoting an organic energizing factor which activates the subject but does not carry with it a "goal set,"—an organic "push" rather than a "pull" (or striving) toward an incentive. When this drive is sufficiently aroused (*e.g.*, through "trophallactic" stimulation in the nest; through picking up or imbibing food at the food-place), the subject is impelled to continue moving until a situation is reached which instigates

a new organic phase (*e.g.*, picking up or imbibing food releasing or regurgitating food, respectively). However, the drive serves to keep the subject moving in the problem situation, and thereby makes possible learning in rote-like fashion a specific route to a food-place (or to the nest). At first the route is established piecemeal in terms of particular segmental adjustments, lacking inter-relationships with other segments or with the end-point. Finally the independent expansion of these local patterns leads to their interaction, a process through which a unitary response to the problem may develop. Psychologically, the ant learns its route very differently than does a man (*i.e.*, a goal-learning man) who learns the way home in a foreign town. To illustrate anthropomorphically, the procedure of the *Formica* subject in mastering her maze path reminds us somewhat of the cheerful but stupid fairy-tale character who had to be off but didn't know where he was going because he hadn't learned the way.

Although we have pointed out some fundamental limitations of the ant's learning capacity, the phenomenon is nevertheless impressive in its own setting and has great adaptive significance. The ability of some members of a colony to master foraging routes obviously is a highly important feature of colony organization, and plays a crucial rôle in the behavior pattern of the species. It must be kept in mind however that this performance, at first sight very similar to trial-and-error learning in higher animals, stands at a definitely lower psychological level than the performance of the mammal at its characteristic best. The fact seems to be that "trial-and-error learning" can be a very different kind of process, according to the nature of the organism in which it occurs. As a broad comparison, we may offer for consideration the obvious fact that while in human trial-and-error learning a "purpose"—*i.e.*, the involvement of goal anticipation—is sometimes a dominant directive factor, this is unfortunately not always the case. Perhaps, by improving our understanding of how some animals learn in the constitutional absence of this great advantage, another step may be taken toward better insight into its proper place in human learning.

REFERENCES

- BIERENS DE HAAN, J. A. 1937 Labyrinth und Umweg. Leiden
- BONNET, C. 1779 Oeuvres d'Histoire Naturelle et de Philosophie T.1^{re} (Observation sur de petites Fourmis, etc.) Neuchâtel.
- BRUN, R. 1914 Die Raumorientierung der Ameisen Jena
- CORNETZ, V. 1910 Trajets de Fourmis et retours au nid. *Inst. Gen. Psychol.*, Mém. No. 2, pp. 167.
- FIELDE, ADELE 1901 Further study of an ant. *Proc. Acad. Nat. Sci.* Phila., 53, 425-449
- FOREL, A. 1874 Les Fourmis de la Suisse. *Nouv. Mém. Soc. Helv. Sci.* Zurich, 26, p. 447.
- FOREL, A. 1908 The senses of insects (tr.) London.
- BRABENSBERGER, W. 1933 Untersuchungen über das Zeitgedächtnis der Ameisen und Termiten. *Ztschr. vergl. Physiol.*, 20, 1-54
- HANNES, F. 1930 Über die verschiedenen Arten des "Lernens" der Honigbiene und der Insekten überhaupt *Zool. Jb., Zool. Physiol.*, 47, 89-150.
- HONZIK, C. H. 1936 The sensory basis of maze-learning in rats *Comp. Psychol. Monog.*, 13, No. 4, p. 113
- HUBER, P. 1810 Recherches sur les mœurs des fourmis indigènes Paris, Genève.
- LUBBOCK, J. (Avebury) 1881 Ants, Bees, and Wasps London
- MAIER, N. R. F. and SCHNEIRLA, T. C. 1935 Principles of Animal Psychology. New York.
- OPFINGER, ELISABETH 1931 Über die Orientierung der Biene an der Futterquelle (Die Bedeutung von Anflug und Orientierungsflug für den Lernvorgang bei Farb-, Form-, und Ortsdressuren.) *Ztschr. vergl. Physiol.*, 15, 431-487.
- PIERON, M. H. 1904 Du rôle du sens musculaire dans l'orientation de quelques espèces de fourmis *Bull. Inst. gen. Psychol.*, 4^{re} Ann., No. 2, 168-186.
- SANTSCHI, F. 1911 Observations et remarques critiques sur le mécanisme de l'orientation chez les Fourmis. *Rev. Suisse Zool.*, 19, 303-338.
- SCHNEIRLA, T. C. 1929 Learning and orientation in ants. *Comp. Psychol. Monog.*, 6, No. 4.
- _____ 1933 Motivation and efficiency in ant learning *Jour. Comp. Psychol.*, 15, 243-266
- _____ 1934 The process and mechanism of ant learning *Ibid.*, 17, 303-328.
- _____ 1941 Studies on the nature of ant learning. I The characteristics of a distinctive initial period of generalized learning. *Ibid.*, 32, 41-82
- _____ 1943 The nature of ant learning. II. The intermediate stage of segmental maze adjustment. *Ibid.*, 35, 149-176.
- SMITH, L. P. 1912 The English Language. London.
- SHEPARD, J. F. 1911 Some results in comparative Psychology *Psychol. Bull.*, 8, 41-42.
- _____ 1914 Types of learning in animals and man. *Ibid.*, 11, 58
- SNYGG, D. 1936 Maze learning as perception. *Jour. Genet. Psychol.*, 49, 231-239
- SPENCE, K. W. 1932 The order of eliminating blinds in maze learning by the rat. *Jour. Comp. Psychol.*, 14, 9-27.
- SPRAGG, S. D. S. 1934 Anticipating responses in the maze. *Jour. Comp. Psychol.*, 18, 51-74.
- TURNER, C. H. 1907 The homing of ants. *Jour. Comp. Neur. Psychol.*, 17, 370-378; 399-401.
- WHITE, R. K. 1943 The case for the Tolman-Lewin interpretation of learning. *Psychol. Rev.*, 50, 157-186.

CONTEMPORARY AMERICAN ANIMAL PSYCHOLOGY IN PERSPECTIVE

T. C. SCHNEIRLA

American Museum of Natural History
and
New York University

At the beginning of the first World War, animal psychology in the United States was in a flourishing condition with promise of continuing the lusty development. There was the *Journal of Animal Behavior*, in its seventh annual volume in 1917 and growing in influence with every number, there were two textbooks *—each an important contribution—and the popularity of the subject in university curricula was increasing. Animal experimental findings were exerting an increasing influence upon general psychology textbooks and courses, especially in discussions of learning. Yet with the last number of their seventh volume the editors of the journal announced its discontinuation ". . . until unfavorable conditions created by the war shall have ceased to exist," papers on animal subjects decreased in number at national meetings, and a decline seemed to have begun that might challenge the possibility of a post-war recovery.

In retrospect, however, that period may be seen as a plateau in the curve of development, as a time of transition and change in the field rather than of arrested development. By examining it in comparison with some of the main aspects of the field in our

* WASHBURN, MARGARET F. 1908 (*Rev.*, 1917, 1926, and 1936) *The Animal Mind*.

WATSON, J. B. 1914 *Behavior: An Introduction to Comparative Psychology*.

own times, perhaps light may be cast on our post-war prospects.

In the past two years many animal psychologists have entered military or other federal service, while numerous others have reorganized their experimental and teaching programs in relation to wartime needs. The number of college courses in the subject has fallen off somewhat, and as an outcome of laboratory reorientations we may expect a decrease in experimental contributions in animal behavior appearing in the *Journal of Comparative Psychology* and the *Journal of Genetic Psychology* within the next year or two. Yet we may rather confidently describe this as a plateau period rather than one of arrested or declining condition. Perhaps we may anticipate with equal confidence that as a transition episode this interval will be followed by notable changes when the onward progress of the subject is resumed. To judge from the history of science in general and from the "plateau" phase of animal psychology during the last war in particular, these changes will occur along lines which have been clearly or latently present in pre-war developments.

From the record we learn that the interval of World War I brought many important new developments in this field, notably the investigations on chimpanzee reasoning by Köhler (as an internee on the island of Teneriffe) and the systematic exploration of brain function and learning by Lashley. Each of these contributions had its foundations in earlier animal work. In Köhler's bibliography were the names of Hobhouse, Haggerty, Rothmann and Yerkes; in Lashley's, Goltz, Franz, and v. Monakov. As other examples, the work of Stone on the basis of mammalian reproductive behavior and Richter's investigations, owed a considerable debt to past research in each case. Also, of course, there was the conditioned-reflex program of Pavlov, which went forward in wartime and even through the critical phases of the revolutionary period in Russia. All of these lines of investigation brought important new contributions by realizing pre-war potentialities, and all of them have profoundly influenced the further development of animal psychology (and of psychology in general) to the present time.

We may expect this process to repeat itself on a new level

in present and future. Important programmatic work continues in many laboratories throughout the world: at the Yerkes Laboratories of Primate Biology in Florida and at the Cornell Experimental Farm, among numerous American institutions which have continued work, and in numerous laboratories in the warring and occupied nations of Europe. Among varied examples, an interesting program of research on the problem-solving capacities of lower-vertebrate animals goes on in the Beritoff laboratory at Tbilisi in the U. S. S. R., and we should not be surprised to find that Kuo in China has kept some important project going even while devoting his main efforts to administrative responsibilities in the national defense program. When conflict ceases there is every reason to anticipate important developments from research now in progress. A glance at the past should prove enlightening as to events in our own country.

The growth of animal psychology in the United States from its beginning somewhat more than 50 years ago has been little short of phenomenal. A measure of this growth in comparison with other branches of psychology in America is given in Table 1, borrowed from Gordon Allport's interesting historical survey.*

TABLE 1
PERCENTAGES OF INVESTIGATIONS UTILIZING VARIOUS SUBJECTS, REPORTED
IN 14 PRINCIPAL PSYCHOLOGICAL JOURNALS (1888-1938)

PERIOD	NUMBER OF PUBLICATIONS	SUBJECT			
		NORMAL HUMAN ADULT	CHILD AND ADOLESCENT	ABNORMAL HUMAN	ANIMAL
1888-1898	86	48.8 *	25.6	2.3	3.5
1899-1908	145	24.9	13.8	8.3	4.1
1909-1918	314	30.6	16.9	4.1	8.9
1919-1928	346	50.18	23.8	2.9	9.0
1929-1938	736	42.9	15.4	3.3	15.2 *

* Percentage of total articles appearing in the given decade.

* BRUNER, J. S., and ALLPORT, G. W. 1940 Fifty years of change in American psychol. *Psychol. Bull.*, 37, 757-776.

These data on publications show that the amount of animal work has increased steadily, with marked advances occurring in the years following 1908 and 1928. The one interval in which no significant increase occurred was the decade following 1918, when child psychology showed its most prominent advance. In view of the fact that these values are percentages of the total number of psychological publications, which rose moderately after 1918 although more slowly than during the preceding decade (which included the war period), it is apparent that even in this "slack" interval there was a substantial increase in animal work. The degree of overall increase in animal investigations after 1910 is emphasized by G. Allport's finding that at the 1914 meetings of the American Psychological Association 11 per cent of the scheduled papers were based on animal research, in contrast to 25 per cent at the 1939 meetings.* The past decade has been a striking development of animal work. In a special survey, Fernberger found animal psychology the most prolific branch of the field from 1932 to 1937, surpassed in output only by "general experimental psychology." ** At the national meetings in 1934, of the 22 sectional programs 5 were devoted to "Animal Psychology," and two others included animal papers. In 1937, at the national meetings, 4 programs were devoted to "Animal Psychology," and animal papers were given in 5 other sectional programs. The evidence shows that animal psychology has had two principal growth phases, in the decade just preceding World War I and in the years preceding the present war.

A comparison of these phases of marked growth discloses some interesting differences in the field during the two pre-war periods in which they occurred. As a basis for the comparison, Table 2 offers a classification of the kinds of animal subjects used in investigations reported in the *Journal of Animal Behavior* during the pre-War I interval and in the *Journal of Comparative Psychology* during the years just preceding World War II.

* ALLPORT, G. W. 1940 The psychologist's frame of reference. *Psychol. Bull.*, 37, 1-28.

** FERNBERGER, S. W. 1938 The scientific interests and scientific publications of the members of the American Psychological Association. *Psychol. Bull.*, 35, 261-281.

TABLE 2
SUBJECTS USED IN AMERICAN ANIMAL INVESTIGATIONS DURING TWO
IMPORTANT PERIODS

PRINCIPAL PUBLICATION	ANIMAL SUBJECT										
	PRIMATES		RAT		OTHER MAMMAL.		INFRA-MAMMALIAN VERTEBRATE.		INVERTEBRATES		TOTAL PAPERS
	No	%	No.	%	No	%	No	%	No	%	
JOUR. OF ANIMAL BEHAVIOR (v. 1-7) 1911-1917	11	6	37	19	30	15	53	27	66	33	197
JOUR. OF COMP. PSYCHOL. (v. 35-42) 1938-1941	31	12	167	66	28	11	14	6	12	5	252

The difference is striking. Judging from journal publications, in the earlier period interest attached to animal behavior widely, with only 19 per cent of the papers based on the rat as subject in contrast to 27 per cent on inframammalian vertebrates and 33 per cent on invertebrates. In the late thirties, however, we find that attention has focussed on the rat as subject, with 66 per cent of the papers, in contrast to strikingly low percentages for papers based on inframammalian animals.

It is clear that there must have been some important differences in the outlook of animal investigators in these two periods. Although the matter is much too involved for adequate treatment here, it is possible to mention certain influences which were clearly involved. Further investigation shows that in the earlier period a large proportion of the work on inframammalian subjects was contributed by zoologists, prominent among whom were Jennings, Wheeler, Mast and Holmes. From these and other zoologists came the largest number of articles on orientation, "instinct," and general behavior, whereas the main contributions of psychologists concerned problems in learning and sensory functions in particular, with far less variety in the animal species used. In animal work at that time the main emphasis was on the qualitative and descriptive type of approach to behavior study, and on objectivity, grounds on which there could be a rapprochement of zoologists and psychologists. Even so, the trend of psychologists toward

quantitative aspects of investigation, toward an emphasis upon methodology, and toward mammalian subjects and the rat in particular, was unmistakeable. The final volume of the *Journal of Animal Behavior*, published in 1917, included nearly twice as many articles involving the rat and the maze as the highest previous total, in the 1915 volume.

Thus the trend toward an increasing emphasis upon methodology and the quantification of results, which became prominent in animal research in the post-war twenties, had reached a fairly advanced point of development prior to the war. The prominence of intelligence testing during the war period, among other wartime influences, served to intensify the emphasis. With this shift toward increased quantification, the descriptive and naturalistic aspects of behavior fell into the background in the animal work of psychologists. An increased specialization of problems and methods augmented the dominance of the rat as the prime subject, and distracted attention from inframammalian forms. In the post-war decade, investigators tended to concentrate on the use of mammalian subjects in specialized investigation of their principal problems, which were early-behavior development, brain function, learning, reproductive behavior, and motivation. All of these specialized projects are readily traceable to a more qualitative research origin in the pre-war period; all of them have continued to the present time as major objects of investigation. Inevitably the specialization of research upon these problems brought animal psychologists into close relationship with mammalian psychologists. Such developments left general behavior studies, and in particular the investigation of inframammalian forms, almost exclusively to zoologists. As one indication of the change from pre-war years, courses in "animal behavior" were offered under Zoology at Chicago, Ohio State, and numerous other institutions,—very different in their content and main program from the "Comparative Psychology" courses of psychologists. The deviation of previously contiguous behavior-study movements in zoology and in psychology was indicated by other changes such as the intensified growth of "ecology" as a special field, prominently involving Shelford, Allee, and others whose early studies

had appeared in the *Journal of Animal Behavior*. As the post-war period lengthened, there became increasingly apparent in American animal psychology a characteristic which committed it more fully to a specialization of subjects and quantitative methods. Even a brief inspection of the record shows that this trait was present in psychological research from the beginning, although it was not very influential until post-war developments brought matters to a focus.

The fact is that American animal psychologists always have been dominantly homocentric in their outlook and have become markedly so within the past two decades. Characteristically, they have tended to apply human concepts to the lower animal, although at the same time they have stressed objectivity in their methods. Paradoxically, the homocentricity has been accompanied by a disdain for anthropomorphism, from which animal psychologists generally consider themselves insulated through training and logical precaution. But the insulation process has been faulty. A consideration of the rise and expansion of "Purposive Behaviorism" and "Operationism" in particular would suggest that the bugbear has been hard by right along.

The allergy to anthropocentrism is an important fact about our behavior and perception which accounts to a considerable extent for the popularity of the rat,—and of Operationism. As an example of how this trait appears to scientists capable of viewing the matter objectively, Köhler has cautioned more than once against studying apparatus rather than the animal or the problem, and against making the point of the problem so inaccessible to the subject that the experimenter's traits influence the results more than do the animal's capacities. And recently, Tinbergen has said

"A further characteristic of modern Behaviourism is its restriction to problems, directly derived from human Psychology. The primary aim apparently is to discover the prehuman in the animals. This is done by focussing attention on the higher, more complicated, processes, while neglecting innate behaviour. The same ideal, however, could also be approached in another way, *viz.*, by studying the animals for their own sake, and after that, tracing the animal in Man." . . . A great part of American Psychology is centered around a few methods which have been

very fertile in the past and are still doing excellent service, and to which it owes much of its splendid progress. However, this has led to an undue emphasis on method, giving it a certain priority above the problem itself. This over-estimation of the value of method finds expression in the centering of problems around mazes, problem boxes and some other methods. . . ." (1942, p. 42).*

This is a fair criticism, and deserves careful consideration. The principal point of the criticism is simply that American psychologists have become so preoccupied with their own functions in the experimental situation that the psychological distance between experimenter and animal subject has become alarmingly great.

There is no question of the rat's monopoly in American animal-psychology laboratories. In the four years preceding 1942 (Table 2), about two-thirds of the articles in the *Journal of Comparative Psychology* and more than half of the papers scheduled for national meetings were based on the rat. The phenomenal growth of the field led to the appearance of four textbooks within five years after 1930, one of them¹ devoted exclusively to the rat and one other² dominated by evidence from rat investigations. (The other two of these books^{3,4} adopted a phylectic organization in which the inframammalian animals also received attention.) Tolman's "Purposive Behavior in Animals and Men" which appeared in 1932 offered a theoretical system based upon the rat almost exclusively, and Skinner's "The Behavior of Organisms," which appeared in 1938, based "a science of behavior" upon operant-conditioning experimentation with the rat.⁵ There are reasons to believe that in the last 10 years the

* TINBERGEN, N. 1942 An objectivistic study of the innate behaviour of animals. *Bibliotheca Biotheoretica*, Ser. D, vol 1, pars 2 39-98.

¹ MUNN, N. L. 1933 An Introduction to Animal Psychology—The Behavior of the Rat. New York.

² MOSS, F. A. (Ed.) 1934 (rev. 1942) Comparative Psychology New York

³ WARDEN, C. J., JENKINS, T. N., and WARNER, L. H. 1934 Introduction to Comparative Psychology. New York.

⁴ MAIER, N. R. F., and SCHNEIERLA, T. C. 1935 Principles of Animal Psychology. New York.

⁵ A bibliography including the "majority of references" on the rat, which appeared in 1930, numbered 1353 items.—As an indication that the American movement had its influence abroad, Bierens de Haan's book "Labyrint und Umweg" (Leiden) which appeared in 1937 should be mentioned. Its subject was maze-learning, and its evidence and discussion centered mainly about the rat.

majority of courses in animal psychology given in American colleges have been devoted largely to mammalian evidence.

Of course, the positive contributions from this intensive concentration on the rat and on methodology have been many, and the influence of the evidence upon psychology in general has been considerable. The rat is now a thoroughly explored subject, through notable contributions on its sensory processes, early development, brain function, reproductive behavior, motivation, conditioned responses, maze-learning and other problem-adjustments, and its higher processes. —Lately the rat has appeared as a useful subject for investigation of abnormal behavior patterns.—The contributions which investigations of the rat have made to general psychological theory of learning and brain function, for example, stand out prominently in most of the contemporary general text-books. Among other important influences of intensive mammalian investigations upon psychology in general, results concerning motivation factors in learning and the objective study of higher processes have been notable.

Progress along these lines has been impressive, yet for the sake of future development it is highly desirable to evaluate this trend toward increasingly intensive mammalian specialization in the light of desirable modifications. The fact is that at this stage we do not have a "comparative psychology," for the truly *comparative* aspects of the science have been progressively minimized the more investigations have focussed upon mammalian subjects and upon problems "close to the human level." As greater emphasis is placed upon instrumentalism in planning experiments and upon highly quantitative procedures in gathering and in treating results, the more "personal equation" enters into the investigation and the less the findings are a function of the animal subject. Such a discipline becomes highly abstract, but the abstractions are introduced from the human level and not really as developments arising inductively from study of the animal's behavior and capacities. Effective experimental control, always desirable, may well be lost in this way. Under the domination of concepts introduced from another level, inductive procedures are minimized or even excluded, and experimenters tend to work

on animal subjects and under experimental conditions that facilitate deductive patterns of thought. When this process becomes chronic it is involuted and Aristotelian rather than inductive and scientific. Teleology may be avoided in the interpretation of results, but anthropomorphism is introduced on the ground floor.

There are numerous paradoxes in the rise of operationism in psychology. A chief one is that although operationists are presumed to be striving toward a thorough-going empiricism, because their instrumental discriminations are made in terms of extra-experimental criteria they are forced into a subjectivism which is essentially non-empirical. Presumably striving toward a more rigid logical procedure in the science, they introduce fallacy from the beginning by framing problem and method in terms of perceptual processes dominated by a field of experience remote from the zone of the subject to be investigated. When it reaches nominalistic extremes, such methodology inevitably loses its natural-science character, for the properties of the instrument and of statistical procedures become the object of investigation rather than the animal subject. It must be recognized that in the decade preceding the present war a non-naturalistic and even anti-naturalistic movement of this kind has developed in American psychology, and that the influence of this trend will be felt strongly in the post-war decade. However, the movement seems to have recently passed its peak, and there are reasons to believe that the future will bring desirable modifications. More than a few experimenters are beginning to realize that while intensive concentration upon the capacities of a caged animal may have great advantages for experimental control, it is a rather artificial way of studying animal psychology in general and does not lead to a *comparative* psychology.

In the years following the present war we may expect closer international relationships to develop in animal psychology as in other sciences. Such interrelationships are bound to be mutually beneficial. European investigators have much to gain through closer contacts with American laboratory and statistical methodology; Americans much from the naturalistic movement which has been maintained in European animal psychology even through

the war period. In the United States, animal psychologists in national service will have been preoccupied for a considerable time with "highly practical" and elementalistic subjects and with statistical (not to speak of military!) procedures. In returning to their laboratories, they will be stimulated in planning new projects by the naturalistic investigations which Continental workers have continued to carry out. On the other hand, the teleological tendencies which have strongly influenced the theorizing of European experimenters should be tempered desirably through closer contact with American objectivism. Along these lines, the organization of an international society of animal psychologists especially to facilitate the exchange of ideas and methods, lectures and material, is a highly desirable post-war project.

Perhaps the most impressive contribution of the early post-war period of World War I was the great fund of evidence derived through research upon the developmental aspects of behavior and the genesis of behavior patterns. This trend toward a dynamic theory of animal personality led not only to searching out the nature of ontogenetic behavior changes in many animals from the early embryonic stages, but also to special investigations of "native" factors in behavior. This work has continued in many laboratories, with real progress toward clarifying the problems of adaptive behavior. Unfortunately in recent years, with increasing emphasis upon the intensive investigation of adult behavior patterns, there has been a swing away from interest in the genesis of individual behavior. There is however an underlying tendency in American animal psychology to give ontogeny its due, and it may well be that a resurgence of this important factor in the earlier development of the field will account for important post-war metamorphoses. Perhaps through such changes in the coming years animal psychology may at length graduate to the status of "comparative psychology."

PART IV

EXPERIMENTAL PSYCHOLOGY

INTERRELATIONS BETWEEN LEVELS OF ASPIRATION, PERFORMANCE, AND ESTIMATES OF PAST PERFORMANCE ¹⁵

JAMES A. BAYTON¹

Virginia State College

INTRODUCTION

The level of aspiration experiment was conceived by Hoppe (17) as a technique for studying dynamic psychological factors which operate in the production of feelings of success and failure. Hoppe's experiments were characterized by a certain degree of informality, the conclusions being based upon the subjects' spontaneous remarks concerning their reactions to the various situations, the manner in which they went at the tasks, and their statements relative to success or failure. The rationale of his approach was that feelings of success or failure would be aroused by discrepancies between aspiration and subsequent performance. If the performance was equal to or exceeded the level of aspiration, success-feelings were presumed to be aroused, whereas if the performance fell below the aspiration, failure-feelings were presumed to result.

An important product of the research by Hoppe was a description of the needs which operate in the determination of a given level of aspiration. A level of aspiration was pictured as the resultant of three forces or needs. First was the need to keep the level of aspiration high. Later workers, notably Gould (11),

¹ The author wishes to express his indebtedness to Dr. Malcolm G. Preston under whose immediate direction this experiment was done, and to acknowledge the benefit of discussions with Dr. Francis W. Irwin and the general encouragement of Dr. Samuel W. Fernberger.

have regarded this need as being an expression either of the individual's desire to do well or of his desire to appear to want to do well. Gould made this distinction on the basis of her discovery that some individuals give levels of aspiration which they do not think they will attain, but do so because they feel they are expected to appear ambitious. It is obvious that this need, no matter what its origin, functions to elevate the level of aspiration. The second need comes from the desire to make the level of aspiration approximate the future performance as closely as possible. Frank (7) presents the view that this need operates to lower the level of aspiration. However, it is not difficult to conceive of two effects upon the level of aspiration of this need, one reflecting the subject's anticipation of improvement and the other his anxiety lest he fall victim to an accident and produce a poor performance. The direction of change in the level of aspiration which resulted from this particular need would depend, of course, upon the relative strengths of the two tensions involved. Finally, there is the need to avoid failure, which tends to exert a downward pull on the level of aspiration, since a greater possibility of failure exists when our aspirations are to a considerable degree in advance of our present level of performance.

The experimental work on the level of aspiration has been characterized for the most part by concern with the analysis of the sources, tension-systems, or needs from which the level of aspiration arises. Actually, however, the situation insofar as the individual is concerned does not terminate with the expression of his aspiration but continues into subsequent activities. If the needs described above have deeper significance than merely being determinants of aspiration, it would be expected that they would exert their influence upon the behavior *following* the statement of aspiration. These needs, as Hoppe (17) originally stated, are not satisfied merely by the statement of the aspiration, but only by what occurs after that. If the individual is to be certain of obtaining satisfaction and feelings of success, must he not *try* to make his performance equal or exceed his level of aspiration? Furthermore, is not the individual whose aspiration is more removed from his past performance level under greater

tension to exert himself than is the individual whose aspiration is relatively close to his past performance level?

When we review the literature on the level of aspiration, we find that despite the fact that the term implies a subjectively determined goal toward which one is striving, there are few experiments which have tested the hypothesis that level of performance is in any way dependent upon the height of preceding aspiration. Filter (5) and Kneeland (19) obtained low correlations between estimates of future performance and performance. Yacorzynski (31) has reported that individuals exerting a minimum degree of effort had higher levels of aspiration than those exerting a maximum degree of effort. It should be noted that Yacorzynski did not concentrate upon performance values, being primarily interested in the quality of the behavior following the expression of the aspirations. He did refer to the performance scores, reporting a tendency for them to be higher for the subjects putting forth the least effort, *i.e.*, those having the higher aspirations. In his experiment, however, this relationship must have been influenced by the fact that minimum degree of effort was indicated by the selection of less difficult methods of completing the tasks.

Notwithstanding this scarcity of experimental work on possible incentive functions of the level of aspiration, investigators have often suggested the possibility that such functions exist, some implying that the level of aspiration is an incentive and others questioning it. Hoppe in his use of the term level of aspiration meant that the subject undertook the task with 'certain demands' being made upon him. He then says that these 'expectations, goal-settings, or demands in connection with one's future performance' (17, p. 10) are expressed in the level of aspiration. Here it has been clearly implied that the level of aspiration is an expression of the manner in which one is approaching a task. Frank (7, p. 119) defined the level of aspiration as 'the level of future performance in a familiar task which an individual . . . *explicitly undertakes to reach.*' (Italics mine)

Gardner (9) has pointed to an important difference in the

use of the term by Hoppe and Frank. Hoppe inferred the implicit level of aspiration from various statements of the subjects and observations of their behavior, whereas with Frank the level of aspiration was the explicit expression, or what the subjects *said* they were aspiring to attain. Subsequent research has followed Frank's procedure almost without exception.²

Gould (12) has referred to the level of aspiration technique (explicit) as a possible method for studying 'how much the future is weighted, how much of an upward pull it has . . .,' but concluded from interviews following her experiment (11) that we have yet to discover the nature of the relationship between implicit goal-strivings and the expressed level of aspiration. Rotter (26) and Gardner (9) have expressed this same doubt about the use of expressed level of aspiration as a measure of the true amount of inner striving. Gardner asks:

"Might not an individual in a task such as dart-throwing entertain at one and the same time a wild hope that he will make a perfect hit and a more prudent hope that he will at least hit the target, with perhaps an additional, self-conscious hope that he will not appear too awkward in the eyes of the experimenter? In other words, is there not considerable likelihood that an individual's aims on a given trial are manifold, fluctuant, ephemeral, and differing qualitatively as well as quantitatively, with those aims which involve a specific score often giving way to aims which cannot possibly be described in terms of score values?" (9, p. 65)

In summary it would seem that considerable doubt can be cast upon the claim that the level of aspiration statement is a

² Some investigators have questioned the key term used in the instructions given in those level of aspiration experiments which follow Frank's procedure. Irwin and Mintzer (18) have indicated the inconsistency which exists among previous workers, since some asked their subjects to state what they *expected* to make, one asked what his subjects *intended* to make, another asked "What *will* you make?", and still another used "What will you *try* to make?" The present experiment employs the most consistently used term—"expect." Irwin in personal communication with the writer has raised the question of whether instructions asking the subjects to state what they *expect* to make have the same meaning as those requiring them to state what they are *trying* to make, and claims that if the incentive value of the level of aspiration is being studied the latter instructions should be used. Since Irwin and Mintzer demonstrate a difference in aspiration statement when their instructions asked for *predictions* as against *hopes*, support is given Irwin's criticism. However, the present experiment is designed to test the incentive value of the level of aspiration as it is generally conceived, and it therefore uses the term most consistently found in the instructions of other investigators.

measure of degree of self-motivation. One of the purposes of the present experiment is to test the hypothesis that performance is related to the relative height of the preceding explicit level of aspiration.

A feature of the experiments which pertains not only to method, but also to the analysis of the deeper structure of the personality forces underlying aspirations, has to do with the adequate control of the instructions so that the subjects can place consistent interpretations upon them, a problem which furnished the ground for the experiment by Irwin and Mintzer previously cited. Preston and Bayton (24) have defined three levels of aspiration. The Maximum level is that level which the subject feels represents his ultimate ability, the Actual level is the score the subject expects to make on the next trial, and the Least level is that score below which the subject is certain he will not fall. That these categories are of more than methodological significance is seen in the fact that they seem to serve at least two purposes for the individual. There is very substantial correlation between the Maximum and Actual levels, but the Least level has a low correlation with them (25). That the Least level is to a considerable extent independent of the Maximum and the Actual levels is also seen in the fact that a social variable tended to affect differentially the Least values as contrasted with those representing the Maximum and Actual levels (24). This differential character of the three levels has been further demonstrated by MacIntosh (20) under conditions which in all important particulars follow the procedure used by Preston and Bayton. Since it has been experimentally shown that these levels of aspiration have different meanings within personality, the important possibility exists that if there is dependence of performance upon aspiration statements it is a function of some particular level or set of levels.

Three considerations lead one to anticipate that any dependence which might exist would be a function of the Actual level of aspiration: (1) the Maximum level of aspiration is by definition removed from present performance in point of time; (2) the Least level is of *negative* valence as a score which the subject

must not make; (3) the Actual level is of immediate concern to the subject and reflects, perhaps, two tensions or needs—the need to do well and the need to avoid failure. Except for those few cases in which the Maximum and Actual levels coincide, it would not seem that the subjects should experience feelings of failure if the Maximum aspiration was not reached on the next trial. On the other hand, both the Actual and Least levels involve the possibilities of feelings of failure. But the Actual level is presumed to express also how well the individual feels he can do in the immediate future. Immediate satisfaction would seem to come not from avoiding the Least level but in attaining the Actual level of aspiration. The individual with the higher Actual level would seem to be under greater tension to make his performance match that aspiration since it is the one toward which attention is immediately directed.

We have been developing the theory that the needs which operate in the determination of the level of aspiration continue to exert their influence upon later performance. There is the further possibility that these needs are not dissipated upon completion of the performance *per se*, but extend into an evaluation of that performance. Before the subjects receive their scores from the experimenter there is evidence that they have tentative feelings of success or failure which are important determiners of the degree of satisfaction experienced. The present writer (24) has observed, in using fictitious scores, that some subjects claim that the score reported to them did not agree with their appreciation of the performance. They seemed to have made estimates of their performances; in fact, it is likely that these subjective estimates began to be formed during the performance. When the score was better than the subjective estimate surprise was expressed. However, most of these expressions came from those who felt their performances had been considerably better than the objective score reported to them.

If in the final analysis the needs are satisfied in terms of the appraisal of the performance, upon which of the preceding events, if any, is the estimate of past performance dependent? Its antecedents are the performance and the statement of the three

levels of aspiration. In the present experiment the measure of performance ability used was scores based upon time, so that the estimate of past performance involved the ability to perceive the passage of time; and it has been shown that the reliability of such estimates is low (29). Kneeland (19) and Hilgard and Sait (15) have reported low correlations between estimates of past performance and the performances themselves.

If the theory presented above holds, *i.e.*, if the subject's definition of immediately doing well and avoiding failure is concentrated in the Actual level, it would seem that the estimate of past performance would be dependent upon that particular level of aspiration. An individual may be expected to desire to obtain satisfaction out of his performance, if possible. Even when one feels that a given performance is poor, there still seems to exist a hope that the performance will turn out to be satisfactory. Therefore, the individual with the higher Actual level of aspiration would be expected to give the higher estimate of past performance, in view of this 'satisfaction-seeking' need which persists beyond the performance. It is the second purpose of this experiment to determine those factors in the level of aspiration situation upon which the estimate of past performance is dependent.

The factor of ego-involvement was recognized by Hoppe (17), and acknowledged by subsequent investigators, to play a critical role in the level of aspiration. The principle which has developed from this approach is that the tension represented in the needs is heightened in terms of the extent to which one's ego becomes involved in the task. Hoppe reported, for instance, that the level of aspiration had meaning only in those tasks which fitted the individual's level of accomplishment and which seemed to test his mettle, and which were not so easy as to seem silly or so difficult as to be impossible of solution. Kneeland (19) listed emotional attitude as a factor in the estimation of the future. Frank (7) found that an attitude of play affected the reliability of the statements of aspiration. Chapman and Volkmann (3), Gould and Lewis (13), Festinger (4), Hilgard, Sait, and Magaret (16), Preston and Bayton (24), and Mac-

Intosh (20) have published results showing that the level of aspiration is a function of the individual's appreciation of his standing in terms of the performance and status of competing groups.

The evidence indicates that behavior in a level of aspiration situation is, in part, a function of the extent to which one feels his personality is involved in the performance of the task. It is possible that if there is dependence of performance upon aspiration it, too, is a function of ego-involvement. Perhaps levels of aspiration given in tasks taken lightly are not of incentive value, while an individual will be more concerned about the relation between his performance and his aspiration for that performance in a situation in which his ego-status is seriously involved. An individual taking a psychological examination which might mean the obtaining of a job must certainly approach that task more concerned about the relationship between his performance and his immediate task-goal than would a student taking the same examination as part of a laboratory exercise. In the same manner, two comparatively simple tasks can offer differential challenge to the individual's status. Of two tasks such as arithmetic and cancellation one would expect that the average person would want to excel in the former, since the psychological importance of skill in cancellation is not immediately apparent but skill in mathematics is commonly accepted as indicating mental prowess. Since the strength of the tensions which exist in any level of aspiration experiment is a vital factor in the outcome, the present experiment includes within its design elements which will permit investigation of the influence of ego-involvement upon the extension of needs beyond the statement of the levels of aspiration.

EXPERIMENTAL DESIGN

General Considerations.—The present experiment was designed to permit a study of the extent to which performance depended upon stated level of aspiration. Comparison of mean performance scores and mean aspirations (based on data from several trials) clearly would not answer this requirement, since such an analysis could not give us information on the cause-effect relationship. Hence, in using it we would be unable to discover whether the aspirations were determining the performances

or the performances were determining the aspirations. To avoid this situation a trial-by-trial analysis was decided upon. By this method we could study each item (performance or estimate of performance) in terms of the events (chiefly aspirations) *immediately* preceding it.

The second requirement of the experimental design was the necessity of controlling variables, other than those relevant to the question under study, which might be influential in determining a difference under examination. The Ss had to be matched in ability to perform the tasks before the introduction of the aspiration variables, since original differences between performers would tend to produce a spurious dependence of performance upon aspiration. In addition to the problem presented by the effect of true levels of performance upon the level of aspiration, there was the problem of the effect of subjective evaluation of that performance. Of two Ss matched in ability to perform a task, one might *think* his last trial was superior and the other that his was inferior. This circumstance could influence the former to give higher levels of aspiration. In order to handle this problem the Ss were matched on the basis of their estimates of past performances, wherever a study was made of the dependence of performance upon a level of aspiration. In addition, age and intelligence were inspected for possible influence upon our situation. Sex and race were controlled by selection of the Ss. In order to exert some control on the region in which the various estimates would fluctuate, fictitious scores were presented to the Ss in the early trials of the experiment.

Subjective involvement was determined by giving Ss brief experience with each of two tasks and asking them to designate the one in which a high degree of skill would be particularly satisfying. The task performed by the S in the experimental session had been previously determined by randomization, so that assignment to a task was not influenced by the choice made by the S. In this manner we had one group working on an ego-involved task and another on a non-ego-involved task.

Subjects.—All Ss were Negro women attending Virginia State College. Each S was tested individually, the experimental session requiring approximately one hour. Three hundred Ss were used, 150 for each task.

Tasks.—The two tasks were selected with the hope that subjective preference for a high degree of skill in one of them would be consistent. One of the tasks made use of arithmetic problems involving such items as the multiplication of four- and five-digit numbers, long division, and addition. The other task was a standard cancellation exercise.

Each S was given one trial on each task (Trials 1a and 1b). The Ss were instructed to perform the tasks and at the end of these preliminary trials were asked, "Which one of these tasks would you prefer to

excel in? In which one would the attainment of a high degree of skill be particularly satisfying?" The task indicated was then considered the ego-involved task. The tasks were alternated in presentation in order to prevent one from occupying a favored position. As we had hoped, all Ss indicated that the arithmetic task was the one in which they would prefer to excel.

Actual Performance Level.—The first part of the experimental session was designed to ascertain the S's level of performance. Following the preliminary trials, seven trials were given. For these trials, and all others throughout the experiment, the time in seconds taken to complete the task was recorded without the Ss being aware of the true values. The Ss were told that accuracy was essential and that this would be checked later.

Estimates of Past Performance.—Fictitious scores were given to each S after each of the above trials with the exception of the last one (after Trials 2 to 7, but not after Trial 8). After Trial 3 the Ss, instead of being given a score, were asked to estimate what that score had been. In order to assist the Ss in evaluating their performance (and later their levels of aspiration) the fictitious scores were presented before Trial 2 as falling in a scale ranging from 0 to 300. The experimenter said, "I am going to tell you your score after you do each trial. I will record the time it takes you but I will have to change it from seconds into scores for statistical reasons. If you don't finish the trial your score will be zero, 150 is the average obtained in other experiments, and a score of 300 is impossible, like running the 100-yard dash in five seconds." At the completion of Trial 8 the experimenter said, "This time you tell me what you think you made." Estimates of past performance were obtained after Trials 8, 9, and 10. No further performance information was given by the experimenter during these trials. Table 1 gives the prearranged scores used. They were the same for both tasks.

TABLE 1
PREARRANGED SCORES

Trial	Scores
2	146
3	148
4	147
5	149
6	149
7	151
8	—

Levels of Aspiration.—Immediately after the estimate of past performance for Trial 8 was obtained, the Ss were instructed to give the Maximum, Actual and Least levels of aspiration for Trial 9. The experimenter was careful that no mention of anything pertaining to aspiration had entered the situation until this point. The instructions for the various levels were as follows: "Now I want you to tell me what you are going to do this next time. However, you will have to give me three different statements. First I want you to tell me the best score you think you could ever make in this task. Then I want you to tell me that score below which you are certain you will not fall if you should have a bad trial. Finally, tell me what you feel you will do this next time, exactly what you expect to make."

Critical Performances.—The above procedure (giving estimates of the previous performance, giving levels of aspiration, and performing the task) was given for Trials 9, 10, and 11. These trials, then, became the critical performances in the experiment.

Influence of Age and Intelligence.—It was necessary to inspect our data in order to determine whether age and intelligence would have to be taken into consideration in our various matching procedures. Table 2 gives the correlations between age and performance, age and levels of aspiration, intelligence and performance, and intelligence and levels of aspiration, for both tasks. These correlations were based upon the data of the 150 Ss in each task. The measure of performance used was the sum of the time in seconds for Trials 6, 7, and 8, these trials being selected because they represented the Ss' ability just prior to the introduction of the experimental conditions. The r 's between age and performance in cancellation and arithmetic were $.22 \pm .05$ and $.02 \pm .05$, respectively. In the case of intelligence, the measure being the IQ's made on entrance as Freshmen, the r 's were $-.05 \pm .05$ and $.13 \pm .05$, respectively. The low r between IQ and speed in cancellation is in keeping with those reported by others (10). The low r for IQ and arithmetic might seem surprising; it is accounted for by the fact that the

TABLE 2
CORRELATIONS BETWEEN AGE AND PERFORMANCE, AGE AND LEVELS OF ASPIRATION,
INTELLIGENCE AND PERFORMANCE, AND INTELLIGENCE AND LEVELS OF ASPIRATION

Variable	Age		IQ	
	Cancellation	Arithmetic	Cancellation	Arithmetic
Σ 6, 7, 8	$.22 \pm .05$	$.02 \pm .05$	$-.05 \pm .05$	$.13 \pm .05$
Maximum	$.02 \pm .05$	$.16 \pm .05$	$.05 \pm .05$	$.04 \pm .05$
Least	$-.15 \pm .05$	$.05 \pm .05$	$.07 \pm .05$	$-.01 \pm .05$
Actual	$.00 \pm .05$	$.09 \pm .05$	$.05 \pm .05$	$.01 \pm .05$

Because of these low r 's it was decided that age and intelligence could be eliminated from consideration in the subsequent analyses.

criterion in the task was time and not accuracy. The problems were simple and, although the necessity for accuracy was stressed in the instructions, only the time in seconds was recorded. The aspiration data used in the correlations were those given for Trial 9. The table reveals that age and intelligence were not related to either the Maximum, Actual, or Least level of aspiration in either task.

RESULTS

Dependence of Performance Upon the Levels of Aspiration.

—In the analysis of the nature of the independence of performance upon the levels of aspiration, the Ss within a given task were matched for previous performance ability and subjective estimate of the performance just preceding the levels of aspiration. Table 3 gives the data showing that the pairs of Ss were adequately matched. In the analysis of the influence of the Maximum level of aspiration in the cancellation task there were 44 pairs of Ss simultaneously matched for previous performance (the sum of the three preceding performances) and estimate of past performance as these values were yielded just prior to Trial 9. The P-value of the mean of the difference between these pairs was between .36 and .35 in the case of the sum of the performances on Trials 6, 7, and 8. The P-value of the difference in variability between the two groups was .46. This indicated that any difference in performance ability which existed prior to the stating of the Maximum aspirations for Trial 9 was due to chance. The P-value was between .18 and .17 for the mean difference in estimate of the performance and the P-value of the difference in variability was between .24 and .23 at Trial 8, showing that there was also no reliable difference between the matched Ss in their estimate of past performance prior to the statement of the Maximum aspirations for Trial 9. In cancellation there were 49 pairs of Ss matched for the two variables in the analysis of the influence of difference in Maximum aspiration for Trial 10 upon performance at Trial 10. The P-value of the mean difference in performance at Trials 7, 8, and 9 was between .35 and .33, while the P-value of mean difference in estimate of what performance at Trial 9 had been was .44. The P-values of the differences in variability for the corresponding

TABLE 3
SIGNIFICANCE OF DIFFERENCES BETWEEN GROUPS MATCHED FOR PAST PERFORMANCE AND ESTIMATED PAST PERFORMANCE

Trial	Cancellation						Arithmetic					
	Number of Matched Pairs	Mn. Diff. in Past Performance	P Diff. in Past Performance	Mn. Diff. in Est. Past Performance	P Diff. in Est. Past Performance	P Diff. in Est. Past Performance	Number of Matched Pairs	Mn. Diff. in Past Performance	P Diff. in Est. Past Performance	P Diff. in Est. Past Performance	Mn. Diff. in Est. Past Performance	P Diff. in Est. Past Performance
Maximum												
9	44	.36 > P > .35	.46	.18 > P > .17	.24 > P > .23	.46	46	.30 > P > .28	.38	.40 > P > .38	.33	.40 > P > .38
10	49	.35 > P > .33	.21 > P > .20	.44	.48	.47	47	.48 > P > .46	.50	.33 > P > .31	.46 > P > .44	.33 > P > .31
11	45	.14 > P > .13	.20	.11 > P > .10	.21 > P > .20	.46	46	.40 > P > .38	.36 > P > .35	.50	.50	.50
Actual												
9	49	.33 > P > .31	.46 > P > .42	.33 > P > .31	.31	.51	51	.15 > P > .14	.21 > P > .20	.33 > P > .31	.35	.40 > P > .38
10	55	.42 > P > .40	.27 > P > .26	.31 > P > .29	.40 > P > .38	.55	55	.38 > P > .36	.40	.29	.46 > P > .42	.46 > P > .42
11	52	.10 > P > .09	.29	.46 > P > .42	.46	.52	52	.44	.40 > P > .38	.48 > P > .46		
Least												
9	40	.24 > P > .23	.31 > P > .29	.15	.36 > P > .35	.45	45	.11 > P > .10	.23 > P > .21	.48 > P > .46	.31	.33 > P > .31
10	47	.46 > P > .44	.50 > P > .48	.23 > P > .21	.40	.52	52	.44 > P > .42	.35 > P > .33	.40 > P > .38	.46 > P > .44	.46 > P > .44
11	52	.14 > P > .13	.30	.29 > P > .27	.42	.47	47	.46 > P > .44	.50	.27 > P > .26	.44	.44

measures were between .20 and .21 and .48. In summary, in the analysis of the dependence of performance on Trial 10 upon the Maximum aspirations for that trial, the Ss were adequately matched for previous performance and estimate of previous performance. Inspection of Table 3 shows that on no trial in either task was there any reliable difference between the pairs of Ss, either in terms of central tendency or variability in previous performance and estimate of previous performance.

The dependence of performance upon the levels of aspiration is given in Table 4. A significant difference in the levels of aspiration was expected, according to the hypothesis of the experiment. In handling the performance scores the following pro-

TABLE 4
DEPENDENCE OF PERFORMANCE UPON LEVELS OF ASPIRATION *

Trial	Cancellation			Arithmetic		
	Mn. Diff in Level of Aspiration	Mn. Diff in Performance and P	% > 0 and P	Mn. Diff in Level of Aspiration	Mn. Diff. in Performance and P	% > 0 and P
Maximum						
9	>.01	.00 (.50)	(.70>P>.50) ⁵⁴	>.01	-3.92 (.16>P>.15)	(.80>P>.70) ⁴⁸
10	>.01	.26 (.40>P>.38)	(.70>P>.50) ⁵³	>.01	-3.51 (.23>P>.21)	(.50>P>.30) ⁴⁵
11	>.01	.11 (.46>P>.44)	(.70>P>.50) ⁴⁷	>.01	-1.50 (.35>P>.33)	(.20>P>.10) ³⁹
Actual						
9	>.01	1.53 (.17>P>.16)	(.20>P>.10) ⁵⁹	>.01	-3.82 (.14)	(.50>P>.30) ⁴³
10	>.01	.35 (.35>P>.33)	(.70>P>.50) ⁵⁴	>.01	-5.54 (.07>P>.06)	(.50>P>.30) ⁴⁴
11	>.01	-.35 (.35>P>.33)	(.95>P>.90) ⁴⁸	>.01	-4.42 (.09>P>.08)	(.99>P>.98) ⁵⁰
Least						
9	>.01	.75 (.35>P>.33)	(.30>P>.20) ⁶⁰	>.01	-1.28 (.38>P>.36)	(.90>P>.80) ⁴⁹
10	>.01	.88 (.09)	(.50>P>.30) ⁵⁷	>.01	-.10 (.42>P>.40)	(.70>P>.50) ⁵⁴
11	>.01	.39 (.31>P>.29)	(.50>P>.30) ⁵⁶	>.01	-.53 (.44)	(.50>P>.30) ⁵³

* *Negative* mean differences in performance indicate that the subject with the higher aspiration produced a faster performance than her match.

cedure was used. Whenever the *S* in the High aspiration group gave a faster performance than her match in the Low aspiration group, that difference was given a minus sign. Therefore, negative mean differences in performance which appear in the table show that the High aspiration group tended to give a faster performance than the Low aspiration group. Such negative differences would support the major hypothesis.

When the 44 pairs of *Ss* matched for previous performance and estimate of past performance in cancellation (Table 3) were divided into groups having high and low Maximum levels of aspiration for Trial 9, the *P*-value of the mean of the difference in Maximum aspiration was less than .01, indicating that they had been reliably divided in Maximum aspiration. In cancellation the mean difference in performance on Trial 9 was .00 seconds with a *P*-value of .50, forcing the conclusion that the groups did not differ significantly in performance on Trial 9. An additional check on this performance was obtained by analysis of the percentage of the cases in which the difference in performance between the pairs exceeded zero. On Trial 9 of cancellation, for the Maximum level of aspiration, 54 percent of the mean differences exceeded zero, showing a slight tendency for more *Ss* in the High aspiration group to perform Trial 9 more slowly than did their matches in the Low aspiration group. The *P*-value of the X^2 for this percentage, however, is between .70 and .50, indicating that the tendency is unreliable. What is true of cancellation is also true of arithmetic, *i.e.*, consideration of the data on the Maximum level of aspiration for the three critical trials reveals that although the *Ss* were reliably divided in terms of aspiration, there was little dependence of performance upon difference in Maximum level of aspiration. However, although none of the differences were reliable, there did seem to exist a slight tendency for high Maximum level of aspiration to be associated with more efficient performance in the ego-involved task.

In the analysis of the dependence of performance upon the Actual level of aspiration, there were 49³ pairs of *Ss* matched for performance on Trials 6, 7, and 8, and for estimate of performance on Trial 8 (Table 3). Table 4 shows that for cancella-

tion there was no significant difference in performance at Trial 9 although there was a reliable difference between the pairs in terms of aspiration. The trend was for those in the High group to produce a slower performance, but the P-value of the mean difference of 1.53 sec. fell between .17 and .16, and the tendency was, therefore, unreliable. No significant difference in performance existed for Trials 10 and 11. In the arithmetic task we found no reliable differences in performance, but the absolute values were somewhat more pronounced in the direction of faster performance being dependent upon higher level of aspiration. The mean differences in performance were -3.82 sec., -5.54 sec., and -4.42 sec. for the three critical trials, and the corresponding P-values were .14, between .07 and .06, and between .09 and .08. The differences in favor of the High aspiration group represent an improvement of approximately 4 percent over the Low aspiration group.

Fisher (6) has given the statistical procedure to be used in the situation in which, although independent tests do not yield a significant P-value, the aggregate of the individual P-values of the tests may be lower than would be expected by chance. The data on the dependence of performance upon the Actual level of aspiration in the arithmetic task lend themselves to this analysis, since the P-value at each trial, although not significant, are quite low, and in addition all differences are in the same direction. The sum of the values of X^2 for each P-value in question is 14.0666 and its P-value (6 degrees of freedom) is between .05 and .02. It is apparent that, considering the data as a whole, the low individual P-values could have occurred less than five times out of 100 by chance.

When we consider the percentages exceeding zero in the case of the Actual estimates in arithmetic, we find that none of the measures are significant, although the direction was toward faster performance for those with high Actual aspirations on Trials 9 and 10. This particular measure is less satisfactory

³ The number of pairs differs from estimate to estimate because of the elimination of those matches who gave identical levels of aspiration.

because it does not take into account either the absolute values of the performances or their variabilities.

In the case of the Least level of aspiration there were no significant differences in performances for the three critical trials in either cancellation or arithmetic. The trend, however, was for those with the higher Least levels of aspiration in cancellation to give poorer performances than their matches in the task. The mean differences in performance for Trials 9, 10, and 11 were .75 sec. (P between .35 and .33), .88 sec. ($P = .09$), and .39 sec. (P between .31 and .29). The percentages which exceeded zero for the three trials were 60 percent (P between .30 and .20), 57 percent (P between .50 and .30), and 56 percent (P between .50 and .30). In the arithmetic task the trend was for those in the High Least aspiration group to give the faster performances. The mean differences in performance were -1.28 sec. (P between .38 and .36), $-.10$ sec. (P between .42 and .40), and $-.53$ sec. ($P = .44$). The percentage exceeding zero was in the same direction only in the case of Trial 9, where it was 49 percent, with P coming between .90 and .80. For Trials 10 and 11, these percentages were 54 percent (P between .70 and .50), and 55 percent (P between .50 and .30).

The data contained in Table 4 lead to the conclusion that performance was not reliably dependent upon differences in Maximum or Least levels of aspiration in either task. There was no dependence of performance upon difference in the Actual level of aspiration in the non-ego-involved task. Although the P -values of the dependence of performance upon the Actual level of aspiration at the individual trials in the ego-involved task were not significant, there was a reliable indication that in terms of the aggregate of the trials higher Actual levels of aspiration tended to be followed by better performances. Apparently, in the case of individual S s who differ in Actual level of aspiration in an ego-involved task no prediction can be made as to subsequent level of performance, but in *groups* of S s we may expect a dependence between performance and Actual level of aspiration.

Dependence of Estimate of Past Performance Upon Levels of Aspiration.—In the analysis of the dependence of the estimates of past performance upon the levels of aspiration, the Ss were matched in the same manner as they were in the analysis of the dependence of performance upon the levels of aspiration. The data showing that the Ss were adequately matched are in Table 3. In the case of the Maximum level of aspiration for Trial 9 of cancellation, the 44 matched pairs showed a mean difference of .09 between the High and Low groups in estimate of performance on Trial 9 with a P-value coming between .44 and .42. For Trial 10 the mean difference was .33 (P between .15 and .14). No analysis could be made for Trial 11, since no estimates of past performance were obtained after the last trial in the experiment. It should be noted that on Trial 10 the mean difference was not reliable (P between .15 and .14), but 62 percent of the cases had differences which exceeded zero and this approaches significance, since P came between .05 and .02. For the arithmetic task there were no significant mean differences in estimate of past performance in relation to differences in the Maximum level of aspiration.

When we consider the estimates of performances 9 and 10 on the basis of differences in the corresponding Actual levels of aspiration we find reliable mean differences for both cancellation and arithmetic. In cancellation the mean differences were .72 and 1.00 for Trials 9 and 10, with the corresponding P-values being between .06 and .04, and less than .01. The P-value of the two trials taken as an aggregate is less than .01. In 73 percent of the cases the difference exceeded zero (P less than .01) on Trial 9, and 68 percent exceeded zero (P less than .01) on Trial 10. In arithmetic the mean differences for the two trials were 1.12 and .74, with P-values of less than .01, and between .02 and .01, respectively. On Trial 9, 71 percent of the differences exceeded zero (P less than .01), and on Trial 10, 65 percent exceeded zero, with P falling between .02 and .01. The direction in every instance was for those who gave higher Actual levels of aspiration to follow them with higher estimates of past performance.

The estimate of past performance was not influenced by the preceding Least level of aspiration on either trial in cancellation. On Trial 9, arithmetic, the mean difference in estimate of past performance which followed the Least level of aspiration for that trial was .62 (P between .07 and .06), 66 percent of the cases had differences which exceeded zero (P between .05 and .02). It will be noted that here, again, we have a case in which reliability is demonstrated in one measure and not in another. However, on Trial 10 both measures were unreliable.

The data on the dependence of estimate of past performance upon preceding levels of aspiration warrant the conclusion that estimates of past performance are dependent upon preceding Actual levels of aspiration, regardless of task. Those individuals who gave the higher Actual levels of aspiration consistently tended to follow them with higher estimates of past performance.

Dependence of Estimate of Past Performance Upon Performance.—In the analysis of the dependence of estimates of past performance upon performance new sets of matchings had to be developed. In this instance it was necessary to match for the performance prior to the critical trial in order that a valid difference in critical performance would be produced. Since the estimate of past performance could be influenced by the previous estimate of past performance the latter had to be considered in the matching. Since it had been demonstrated that the estimate of past performance was influenced by the preceding Actual level of aspiration the Ss had to be matched in terms of that measure. After matching for these three variables, the pairs were divided into High and Low performance groups. Since no estimate of past performance was obtained after Trial 11 the analysis considers only Trials 9 and 10. In cancellation for Trial 9 there were 47 pairs matched for the variables outlined above. The P value of the mean of the differences for previous performance (Trials 6, 7, and 8) was between .35 and .33 and the P value of the difference in variability was between .40 and .38. In the case of the estimate of what performance at Trial 8 had been the P value of the mean difference fell between .13 and .12 and

the P value of the difference in variability between .27 and .26. For the Actual level of aspiration for Trial 9 the P value of the mean difference fell between .48 and .46 and the P value of the difference in variability was .50. It is apparent that the Ss were matched on the three variables both in terms of central tendency and variability. The P value of less than .01 for the mean difference at Trial 9 showed that the Ss were reliably divided in terms of that performance. The mean difference of the estimate of what performance at Trial 9 had been was $-.11$ with the P value between .40 and .38, and 49 percent of the cases gave a difference which exceeded zero, the P value being between .90 and .80. These data force the conclusion that the estimate of past performance in this case was not dependent upon the performance itself. Consideration of the data for Trial 10 cancellation and Trials 9 and 10 arithmetic shows that on one of these trials was the estimate of past performance influenced by the performance being estimated.

Dependence of Estimate of Past Performance Upon Previous Estimate of Past Performance.—The variables, other than the previous estimate of past performance, which immediately preceded the estimate of past performance at a given trial were the performance and the levels of aspiration for that performance. It has been shown that of these latter variables the only one which exerted an influence on estimate of past performance was the preceding Actual level of aspiration. Therefore, in studying the dependence of the estimate of past performance upon the preceding estimate of past performance it was necessary only to match for the Actual level of aspiration. In cancellation there were 31 pairs matched for the Actual level of aspiration for Trial 9. The P value of the mean of the difference between these pairs was .50 and the P value of the difference in variability was between .40 and .38, showing that any difference between the pairs was due to chance. The two groups were reliably divided in terms of the estimates of what performance on Trial 8 had been since the P value of the mean difference of that estimate was less than .01. The mean difference of the estimates of performance on Trial 9 was .81 with a P value between .04 and .03,

and 66 percent of the cases gave a difference which exceeded zero, but the X^2 of this percentage was not quite reliable, the P value falling between .10 and .05. However, for Trial 10 cancellation and Trials 9 and 10 arithmetic both critical measures were reliable, all of the P values being less than .01. It is evident that the estimate of what performance was on a particular trial was dependent upon the estimate of what performance had been on the previous trial. The direction of this dependence was for high estimate of past performance to be associated with high previous estimate of past performance.

DISCUSSION

The theory was advanced that the needs to do well and to avoid failure should operate in such a manner that the individual with the higher Actual level of aspiration should have a greater incentive to perform more efficiently than one with a lower Actual level of aspiration, particularly in an ego-involved task. The results of this experiment justify such a conclusion only so far as group results are concerned.⁴

We offer the following explanations of the fact that the dependence of performance upon the preceding Actual level of aspiration was so small in amount. It is possible, first, that the arithmetic task did not create enough ego-involvement to produce a striking effect. It is possible, secondly, that these explicit statements of aspiration are not reliable measures of subjective goal-striving. This view is in keeping with opinions previously cited. We must admit that the differentiation of three levels of aspiration might still not be refined enough for the solution of our present problem. For instance, following Gould (12), the three levels can still be either expressions of desire to do well or of merely wanting to appear ambitious. Perhaps, as Irwin suggests, a key to future experimentation would be the shift from asking for *expectations* concerning future performance to asking the Ss

⁴ Although Fryer (8) has made a distinction between interests and motivation, it would seem that our results are to some extent comparable to those obtained in experiments dealing with the relationship between interest and achievement. The results in this field have failed to reveal any high correlations between interests and ability (8). We are comparing here the two approaches because they are both based upon statements which are subjectively determined.

to state what they are *trying* to achieve. In the final analysis, from a practical point of view, future experimentation should acknowledge both alternatives.

Turning now to a consideration of all of the experimental results which bear upon the relations between aspirations, performance, and estimates of past performance we shall attempt to show how they may be used to develop a general theory of the behavior of the variables involved. We shall consider first the extent to which the Actual level of aspiration and the estimate of past performance reflect the operation of the same forces.

That some difference exists between these two variables is evident in the fact that the estimates of past performance are less variable than the Actual levels of aspiration.⁵ This suggests that the estimate of past performance is more in the nature of a judgment than is the Actual level of aspiration.⁶ The estimate of past performance, as an estimate of an event which has already occurred, is presumably based to some degree upon objective information already received. The use of fictitious scores results in all Ss' estimating from the same objective background, a circumstance creating homogeneity of estimation of past performance. On the other hand, the Actual level of aspiration is an estimate of an event in the future, and therefore has more freedom of determination.

Although the Actual level of aspiration and the estimate of past performance have been shown to be dissimilar in the above respects, there are many similarities between the two measures which suggest that to a considerable extent they are influenced by the same forces. The most striking fact relating the Actual level of aspiration and the estimate of past performance is the fact that the latter is heavily dependent upon the Actual estimate regardless of task. Apparently individuals tend to feel that their

⁵ The data comparing the variabilities and distributions of the Actual levels of aspiration and the estimates of past performance are in the author's doctoral dissertation "Interrelations between Levels of Aspiration, Performance, and Estimates of Past Performance," on file in the Psychological Laboratory, University of Pennsylvania.

⁶ McGehee (21) has indicated that although the level of aspiration is to some extent a judgment it is more a product of the 'ego-level' than of the process of judging.

performances have come within the region of their immediate aspirations in both ego-involved and non-ego-involved tasks. The need to do well and to avoid failure, which is concentrated in the Actual level of aspiration seems to extend into the estimate of past performance, which can be viewed as an expression of a 'satisfaction-seeking' need in which one hopes he has *done* well and has *avoided* failure.

Further evidence in support of the position that the Actual level of aspiration and the estimate of past performance are the products of similar needs is seen in other striking similarities in their behavior. The variabilities of the two measures, although different, are of the same order of magnitude, the sigmas ranging from 2.38 to 3.34; on the other hand, the sigmas of the Maximum estimates range from 13.71 to 18.41, and those for the Least estimates from 13.82 to 32.85. Finally, the distributions of the Actual estimates and the estimates of past performance are both approximately symmetrical whereas the distributions of the Maximum and Least estimates appear markedly skewed.

In addition to the fact that the needs which influence the Actual level of aspiration and the estimate of past performance are highly similar the evidence suggests that these needs tend to persist in time. The reliability coefficients reported by Frank (7) and Preston and Bayton (24) indicate that there is a within-task tendency to place the levels of aspiration at characteristic heights. In other words, there are individuals who give characteristically high aspirations and others characterized by low aspirations. The results of the present experiment show that the same phenomenon exists in respect of the estimates of past performance. When the Actual level was held constant, and with performance *per se* playing no part, those subjects with the higher estimates of past performance on one trial maintained higher estimates of past performance on the next trial.

The foregoing discussion appears to justify the following theory of the nature of the interrelations between levels of aspiration, performance, and estimates of past performance. The individual feels a need to do well and a need to avoid failure. These needs find critical expression in the Actual level of aspira-

tion. The Maximum level of aspiration also reflects these needs but is not of immediate incentive value because it is by definition removed in point of time and refers to ultimate aims, whereas the Actual level is characterized by immediacy.⁷ The Least level defines complete failure and is an avoidance goal, but success-feelings would not be gained merely by performing above the Least estimate, but would rather be gained in achieving one's immediate goal, namely, the Actual level of aspiration. The needs which are reflected by the Actual level of aspiration are not momentary but continue to exert their influence upon subsequent events, including the formation of new levels of aspiration and new estimates of past performance. In ego-involved tasks there tends to be a demand on the individual to satisfy these needs by the release of energy commensurate with that necessary to attain the goal expressed in the Actual level of aspiration. After the performance, regardless of the degree of ego-involvement and its determination of the release of energy in the performance, these needs are reflected in the attitude one has toward his performance which find expression in the estimate of past performance. Here the individual hopes that he has done well and avoided failure.

It is not uncommon to find individuals evaluated in terms of their expressed levels of aspiration. Personnel workers may be influenced by the aims stated by job applicants. We seem now to have objective data which bear upon the significance of statements of this kind. For one thing, the interpretation of the real incentive value of the aspiration must be made in terms of the meaning of the task to the individual. Apparently we can expect a relation between aspiration and achievement only in those tasks in which the person's ego is deeply involved. However, even in an ego-involved task it is our *immediate aspirations* which are of incentive value. It would seem, then, that the most important subjective areas which should be explored in this appraisal should be those having to do with the immediate levels of aspiration.

In conclusion I would like to point out that among psychologists there has been a growing feeling of dissatisfaction with

⁷ Thomsen (28) has advanced the view that those individuals who expect maximum achievements in the immediate future are particularly courting unhappiness.

static, questionnaire approaches to the study of personality (30). Recent years have been characterized both by demands, and attempts to satisfy these demands, for the analysis of personality manifestations through the use of functional and rigidly experimental methods. No one has denied that Hoppe introduced an important problem in personality and recognized most of the major factors which influence our self-motivated behavior. Subsequent experimentation has further refined both the methods and the concepts involved until it seems that today substantial progress is being made toward the achievement of experimental methods for the study of personality.

SUMMARY

The level of aspiration has been interpreted as being an expression of certain needs: the need to do well (or to appear to be wanting to do well); the need to approximate the future performance; and the need to avoid failure. Research up to the present time has been mainly concerned with the analysis of factors which influence the operation of these needs in the determination of the level of aspiration. The present experiment attempted to investigate the nature of the influence of these needs upon the events following the statement of the aspiration, namely, the performance for which the aspiration was given, and the individual's estimate of that performance.

There are at least three levels of aspiration available to the individual. The Maximum level of aspiration represents ultimate ability, the Least level of aspiration is the score below which the individual is certain he will not fall, and the Actual level of aspiration is the score which the subject expects to make on the next trial. The hypothesis was presented that in an ego-involved task the needs expressed in the Actual level of aspiration would continue to exert their influence in such a manner as to produce a dependence of height of performance upon the height of the Actual level of aspiration for that performance. A further hypothesis was presented to the effect that satisfaction-seeking arising as a consequence of the needs would operate in such a manner as to make the height of the estimate of past performance depen-

dent upon the height of the Actual level of aspiration for that performance.

The present experiment was designed so as to permit a trial-by-trial test of the two hypotheses in an ego-involved task and a non-ego-involved task. The results support the view that the influences which find expression in the level of aspiration continue to be operative upon the events which follow the statement of the aspiration. Although the data were unreliable for individual trials, the data for the aggregate of the three critical trials, in the ego-involved task, revealed a reliable tendency for those subjects with higher Actual levels of aspiration to follow them with better performances. Also, regardless of the nature of the task, there was a reliable tendency for those subjects who gave higher Actual levels of aspiration to follow them with higher estimates of past performance.

BIBLIOGRAPHY

1. ANDERSON, H. H., & BRANDT, H. F. Study of motivation involving self-announced goals of fifth grade children and the concept of level of aspiration. *J. soc. Psychol.*, 1939, 10, 209-232.
2. BAYTON, J. A. The level of aspiration technique as an experimental method for the study of morale. *Virginia J. Sci.* (Abstract, in press.)
3. CHAPMAN, D. W., & VOLKMANN, J. A social determinant of the level of aspiration. *J. abn. soc. Psychol.*, 1939, 34, 225-238.
4. FESTINGER, L. Wish, expectation, and group standards as factors influencing levels of aspiration. *J. abn. soc. Psychol.*, 1942, 37, 184-200.
5. FILTER, R. O. Estimates of the amount of work one can do. *J. appl. Psychol.*, 1927, 11, 58-67.
6. FISHER, R. A. *Statistical methods for research workers*. London: Oliver & Boyd, 1936.
7. FRANK, J. D. Individual differences in certain aspects of the level of aspiration. *Amer. J. Psychol.*, 1935, 47, 119-128.
8. FRYER, D. A. *The measurement of interests*. New York: Henry Holt, 1931.
9. GARDNER, J. W. The use of the term "level of aspiration." *Psychol. Rev.*, 1940, 47, 59-68.
10. GARRETT, H. E., & SCHNECK, M. R. *Psychological tests, methods, and results*. New York: Harper & Brothers, 1933.
11. GOULD, R. An experimental analysis of "level of aspiration." *Genet. Psychol. Monogr.*, 1939, 21, 3-115.
12. GOULD, R. Some sociological determinants of goal strivings. *J. soc. Psychol.*, 1941, 13, 461-473.
13. GOULD, R., & LEWIS, H. B. An experimental investigation of changes in the meaning of level of aspiration. *J. exp. Psychol.*, 1940, 27, 422-438.
14. HERTZMAN, M., & FESTINGER, L. Shifts in explicit goals in a level of aspiration experiment. *J. exp. Psychol.*, 1940, 27, 439-452.
15. HILGARD, E. R., & SALT, E. M. Estimates of past and future performance as measures of aspiration. *Amer. J. Psychol.*, 1941, 54, 102-108.

16. HILGARD, E. R., & SALT, E. M., & MARTARET, G. A. Level of aspiration as affected by relative standing in an experimental social group. *J. exp. Psychol.*, 1940, 31, 411-421.
17. HOPPE, F. Erfolg und Misserfolg. *Psychol. Forsch.*, 1930, 14, 1-62
18. IRWIN, F. W., & MINTZER, M. G. Effect of differences in instructions and motivation upon measures of the level of aspiration. *Amer. J. Psychol.*, 1942, 55, 400-406
19. KNEELAND, N. Self-estimates of improvement in repeated tasks. *Arch. Psychol.*, 1934, 163, 75.
20. MACINTOSH, A. Differential effect of the status of the competing group upon levels of aspiration. *Amer. J. Psychol.*, 1942, 55, 546-554
21. MCGEHEE, W. Judgment and level of aspiration. *J. gen. Psychol.*, 1940, 22, 3-15.
22. PENNINGTON, L. A. Shifts in aspiration level after success and failure in the college classroom. *J. gen. Psychol.*, 1940, 23, 305-313
23. PRESTON, M. G. Use of the coefficient of correlation in the study of the D-score for the level of aspiration. *Amer. J. Psychol.*, 1942, 55, 442-446.
24. PRESTON, M. G., & BAYTON, J. A. Differential effect of a social variable upon three levels of aspiration. *J. exp. Psychol.*, 1941, 29, 351-369
25. PRESTON, M. G., & BAYTON, J. A. Correlations between levels of aspiration. *J. Psychol.*, 1942, 13, 369-373.
26. ROTTER, J. B. Level of aspiration as a method of studying personality. *Psychol. Rev.*, 1942, 49, 463-474.
27. SEARS, P. S. Levels of aspiration in academically successful and unsuccessful children. *J. abn. soc. Psychol.*, 1940, 35, 498-536
28. THOMSEN, A. Expectation in relation to achievement and happiness. *J. abn. soc. Psychol.*, 1943, 38, 58-73.
29. TIFFIN, J., KNIGHT, F. B., & JOSEY, C. C. *The psychology of normal people*. Boston: D. C. Heath, 1940
30. WOODWORTH, R. S. The adolescence of American psychology. *Psychol. Rev.*, 1943, 50, 10-32.
31. YACORZYNSKI, G. K. Degree of effort: III. Relationship to the level of aspiration. *J. exp. Psychol.*, 1942, 30, 407-413.

OCULAR PHOTOGRAPHY: A SCIENTIFIC APPROACH TO THE STUDY OF HUMAN BEHAVIOR

HERMAN FRANCIS BRANDT

Drake University

Scanning the history of science in its manifold aspects we are impressed with the emphasis on its respective phases in different periods of its development. We have had the Era of Astronomy, Physics, Chemistry, and Biology. If the history of science is reviewed five hundred years from today, it is likely the Twentieth Century will stand out as the Psychological Era. Not that these established sciences are passing, but rather that they are finding fulfillment in this new field known as the Science of Human Behavior. Psychology, because it employs the same scientific methods that constitute the basis of other sciences, is considered today on a par with other fields of investigation.

The *physicist*, for example, analyses to the minutest detail the composition and organization of matter in all of its diverse forms and relations in his quest for the cause of physical behavior, and returns with the exclamation, "I cannot find energy." This discovery is not an admission of defeat in scientific research, but rather a confession that reality lies in the realm of the intangible.

The *biologist* in search of the basis of the physiological behavior of man and animals dissects the tissues of the organism to discover their structure and function and their relation to the whole. He, too, returns with the same refrain, "I cannot find life."

The *psychologist*, no less, after a careful examination of the structure and function of the nervous system and the evaluation

of behavior under controlled conditions, comes to the same conclusion, namely, "I cannot find the mind."

Whether we seek an explanation for *energy*, *life*, or even *mind* itself, we discover the nature and characteristics of these intangibles through behavior of some kind. On this basis, we conclude that psychology, whose aim is to interpret the manifestations of human nature, gains its information about the mind by evaluating the behavior resulting from adjustment.

If behavior is an answer to wants, and if mind is discovered by a careful evaluation of such behavior, it becomes apparent that the study of the human eye as a motor organ becomes the logical approach to the study of mental processes.

To photograph the unconscious adjustments of the human eye, and thus reproduce an ocular pattern which becomes a permanent record for scientific analysis, is both an art and a science. Such a record yields data indispensable in the interpretation and evaluation of human conduct.

What we know about the human mind has been revealed through behavior of some kind; and no single organ of the human mechanism has as truly a sensory and motor function as has the eye. This does not mean, nor does it even imply, that the eye is the only motor organ which reveals to us the mysteries that lie back of behavior. The glands and muscles of the entire body are all necessary for this evaluation. For the purpose of this paper, however, the writer will confine himself to a study and explanation of the human eye and its manifestation of the human mind.

The eye as a sensory-motor organ, next to our heart and mind, is our most coveted possession. When we realize that during normal observations the eye fixates from 150 to 350 times per minute, and that an equal number of excursions are made in the same period of time, we are impressed with the importance of this, our major sensibility. These unconscious adjustments of the eye to the demands of the physical conditions of the environment and the psychological demands make the eye the coveted avenue to the study of the higher mental processes.

Psychological Problems: To gain adequate information from the subject without causing him to become self-conscious in the experimental situation and to learn from the subject the exact nature of his experience have always been the two major problems in the analysis of psychological processes of individuals. If verbal or written reports are required, a premium is placed on introspection; the data thus obtained is more or less subjective, and consequently the entire experiment is affected by it.

In order to make an accurate analysis and diagnosis in the field of visual science, adequate instruments of precision are indispensable. Just as in the evolution of civilization, the discovery and application of tools made for culture and progress, so in experimental psychology instruments of precision, properly employed, have led to far-reaching psychological advances.

Ocular Photography: Ocular Photography as a laboratory technique has minimized the two major difficulties stated earlier. Individuals thus examined are not conscious of their eye movements, as they are of their responses when required to speak, walk, or perform under observation. Since the eye movements are unconscious adjustments of the individual to the demands of certain stimuli or psychological needs, we as experimenters find ourselves in a fortunate position to catch with instruments of precision these honest, frank, and trustworthy behavior patterns for our analysis.

Eye movements, serving as objective symptoms of perceptual processes, are readily discovered and effectively located although the underlying motives may not be known at the time to either subject or investigator. Whether the subject is studying a problem with the intent to solve it, or whether he merely looks at a picture for appreciation, the eyes follow a definite course as truly as a stream winds its way toward a certain destination. The underlying causes for a given type of behavior are none too well known, but by proper and painstaking analysis, research will discover the underlying causes of eye movement behavior in their relation to the environment. It recognizes the possibilities in the field, if the technique is properly applied.

As a technique, Ocular Photography is not new, but its application and the subject matter studied are a recent development. Preceding the present eye camera, records of eye movements were obtained by studying after-images; by attaching apparatus directly to the eye; by direct observation; by recording eye movements, using mirrors and eye lid attachments; by photographing horizontal or vertical eye movements only. All these devices were designed for the purpose of charting the course of the eyes in motion.

Points of view: In attempting to establish a point of departure for seeking relationships between ocular performance and the psychological implications applied to novel situations, one is confronted with two, or possibly three, theories. One theory holds that poor central processes are due in part to inefficient eye-movement habits, and that special training in eye-movements would result in a more efficient procedure.

Another school contends that eye-movement habits are not the cause of a particular mental activity, but rather that central processes determine the characteristic nature of the eye movements themselves.

A third point of view implies that there exists a functional relationship between ocular movements and central processes. This view recognizes both receptor and effector as a necessary part of the mental equipment to produce what is commonly known as mental activity.

Theoretically, as well as practically, one encounters innumerable difficulties in ascribing a relative degree of cause and effect to respective media of a process. However, in ascribing to the receptor and the effector mutual inter-dependence, one is not likely to lose sight of the importance of the sensory channel in presenting subject matter to the individual or to underestimate the importance of the motor responses in conscious activity. Although the author of this study believes that the eyes stand ready to serve the central processes and do their bidding, he nevertheless contends that inefficient central processes over a period will result in faulty eye-movement patterns which will result in inefficient observation and learning.



Fig. 1. A portable mono-film eye-movement camera and subject observing a two-page spread of editorial copy.

DETERMINERS OF OCULAR PERFORMANCE

As already indicated, the variation of the location, sequence, frequency, and duration of fixations of eye movements depends, as does the distance and direction of excursions, upon conditions of the individual and his environment. The intellectual capacity of the individual, his past experiences, formal training, and the purpose of the observer at the moment, all in one way or another play a definite role in determining his ocular performance. The ocular pattern, when recorded by a camera, becomes a permanent record of the performance for a given task and stands as evidence of the nature of the sensory and motor experience of the observer. Ocular patterns become meaningful only when fixations, time, excursions, and space are adequately measured by laboratory instruments; and when the interpretation is based on such accounts.

It is logical to think of the mind in terms of physical dimensions when we remember that our mental content is composed of sensory and motor experiences. These serve as memory traces and are retained in the form of intensity, extensity, protensity, or a quality of some kind. It is only fair to believe that our ap-

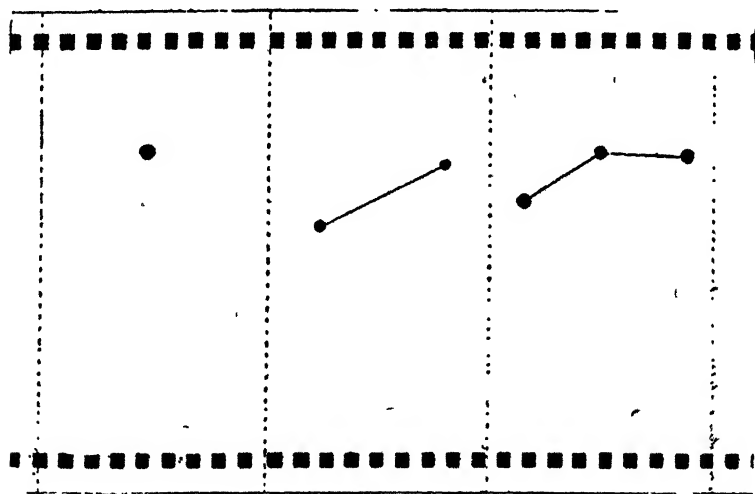


Fig. 2. Each dot represents an eye fixation on the film, resulting from observation of a given area.

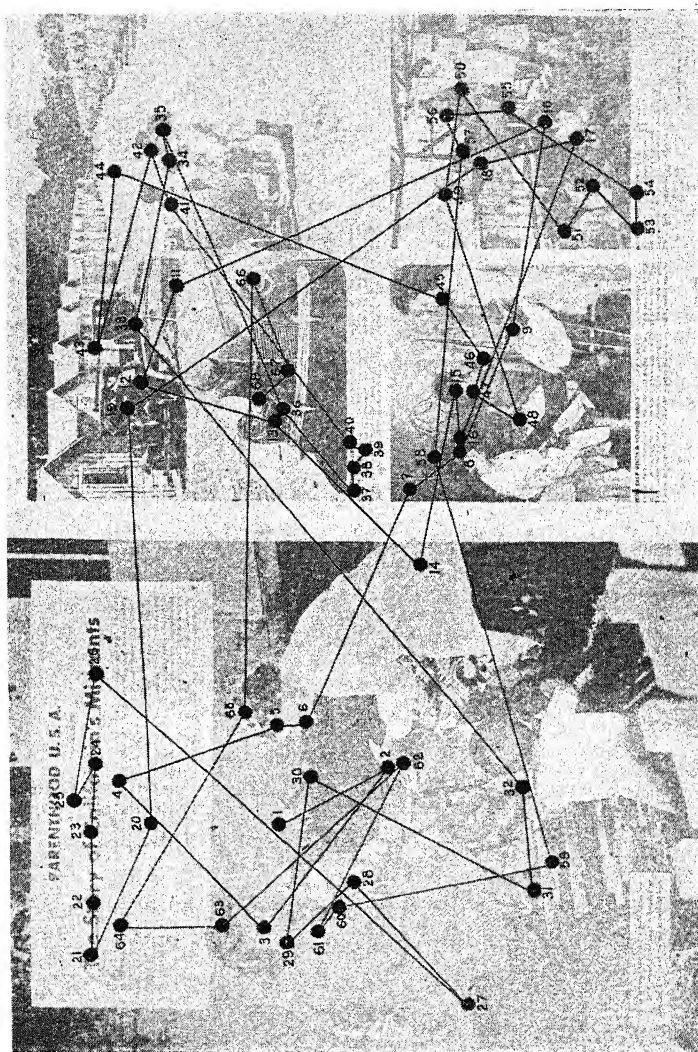


Fig. 3. An ocular pattern of subject in Fig. 1.

proach to the study of the human mind should follow directly the course charted by our response to these novel stimuli present.

THE BI-DIMENSIONAL EYE-MOVEMENT CAMERA

Since 1937, when the author first released information relative to a new mono-film bi-dimensional camera, the eye movements of thousands of subjects have been photographed.

The new instrument of precision developed by the author is a camera equipped with a 35mm. film moving intermittently at a constant speed.

This camera provides an exact record of the ocular pattern resulting from the observation of a given field. It records the duration, location and sequence of every fixation as well as the direction and distance of every excursion. The photographic record when projected upon the originally observed content yields exact information relative to the amount of time spent in respective areas.

Operation: The operation of the camera is based on the principle that the human eye, in focusing a certain point, rotates in such a way that the line of regard strikes the fovea of the retina. The fovea is the most sensitive portion of the retina, although a very small area is the point of clearest vision.

Two small beams of light emanating through small openings from tubular housings of the camera are reflected from the two eyeballs and are carried down small telescopic tubes, provided with achromatic lenses.

By means of this arrangement, motion picture film, stopping intermittently to catch the eye in each new fixation, photographs a tiny dot on the film. Every fixation of the eye changes the high light on the cornea and consequently is focused at different points on the sensitized film.

Projection: The film, after being processed, is placed in a high powered stereoptican and projected on a field equivalent to the pages originally observed by the subject. Since each eye fixation is recorded on the film as a single dot, the analysis of the record is a simple matter. As the eyes move over test mate-

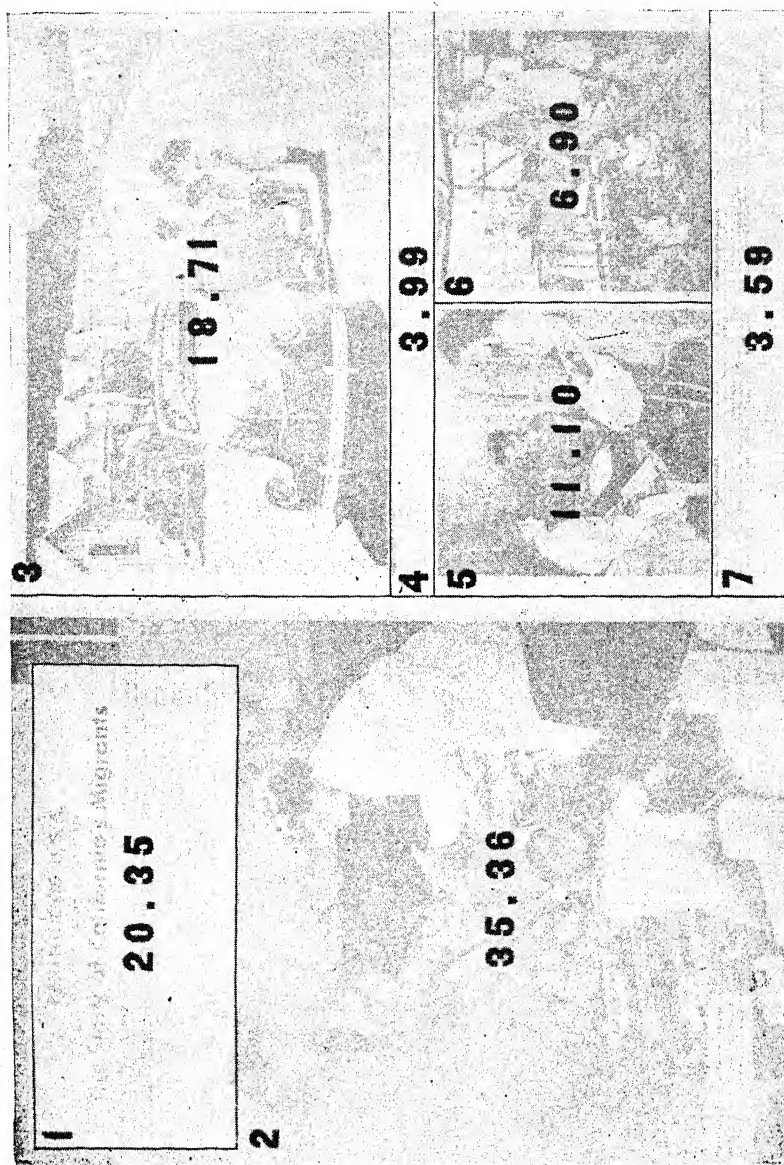


Fig. 4. Relative time in percent spent by 100 subjects in observing a double-page spread in a magazine.

rials, dots are recorded on the film in exact relationship to the points where the subject fixated on the page.

If, for example, a fixation falls within a certain area of a given field, the eye fixation as projected will fall within that area. Areas are constructed for each type of material projected, and the relative time in percent devoted to respective areas may be calculated. (See Fig. 4.)

Ocular Patterns: Figure 3 illustrates a typical eye pattern of a subject looking at a two-page spread of pictorial copy. The location of each fixation is determined by the relative deviation of the eye in its rotation from the original reference point, and the duration of fixations are measured by the number of frames the film moves per second. Ocular patterns represent the duration, location and sequence (numbers in Figure 3 indicate the sequence) of every fixation as well as the distance, direction; and frequency of every excursion or eye movement.

Application: This mono-film bi-dimensional camera is portable in construction, compact in organization, and streamlined in design. It provides an efficient, flexible, and reliable instrument for research in laboratories, schools, and offices, alike. It is economical in operation and practical for varied types of visual research.

Due to the demand for an instrument which will accurately and efficiently photograph eye movements in the bi-dimensional plane, this camera was constructed. Whether the investigator is interested in the attensity of physical variables such as position, size, color, isolation, etc., or whether he desires information relative to the interests, intelligence, and purposes of different individuals, the camera provides a convenient, economical and scientifically accurate apparatus.

With a bi-dimensional camera which provides an objective record of ocular performance, a vast field of information heretofore the subject of guess and speculation can now be investigated scientifically.

Findings of the Visual Research Laboratories have revealed that ocular patterns, resulting when reading a book, looking at advertisements and pictures, or solving a problem, are indicative

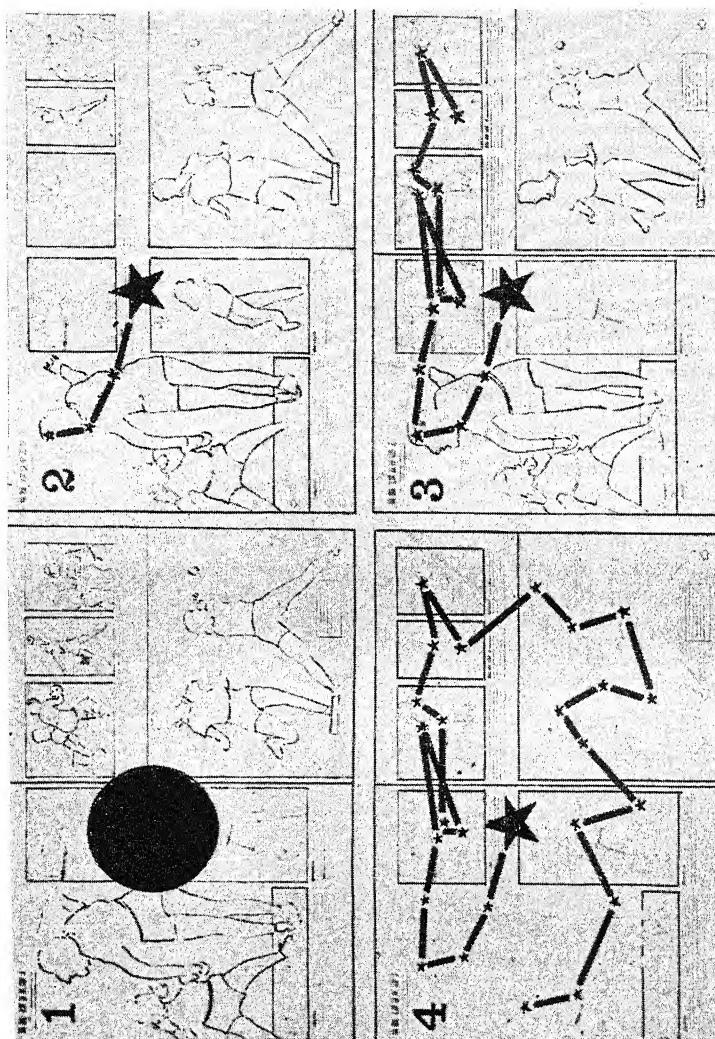


Fig. 5. Location of initial fixation and course of a typical ocular pattern. (Layout and course employed strictly for illustrative purposes.)

of the mental processes involved. The confidence of being able to evaluate the influence of physical and psychological variables by means of this technique has prompted the author and his associates to spend much time and money to perfect the necessary equipment and technique.*

OCULAR PATTERN AND THEIR COMPONENTS

In studying the ocular performance of the human eye it is imperative that we understand the characteristic eye movement tendencies. In order to better understand these tendencies we will consider briefly their various components.

Fixations: Contrary to common belief, the eye does not glide over a picture or along a line of printed matter. It covers the area in jumps and stops which are known as fixations, and excursions, or eye movements. These equal about 240 per minute or 14,400 fixations per hour. If these eye movements were converted into sound, so that we could hear ourselves and others see (as we now can), it would be like listening to the ra-ta-ta-tat of a machine gun. Under such a demonstration as this, one would be even more impressed with the consistency or inconsistency of the ocular performance of the individual.

The eyes at work provide us with a different concept of their function and importance than we are able to obtain when observing these sensory motor organs in a static state. The duration of successive fixations varies from five-hundredths of a second to a second or more, depending on the kind of material observed and on the ability, interest, and habits of the observer. Clear vision takes place only when the eye fixates, the eye being temporarily blinded while in motion.

Excursions: The excursions, so called, are the movements of the eye from one fixation to another. The distance of the respective eye-movements during the observation of a given field varies from a few millimeters to a distance stretching completely across an entire observed area.

* Funds for the construction and development of the cameras and research were provided by Look, Inc., New York, N. Y.

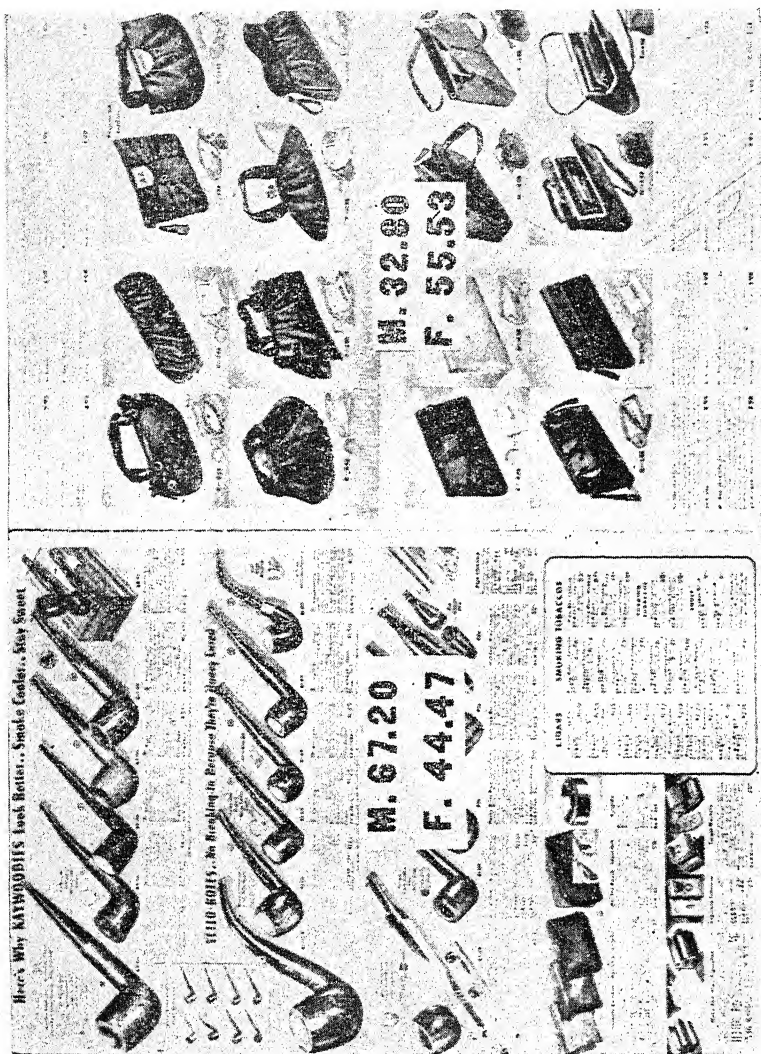


Fig. 6. Relative time (in percent) devoted by 50 male and 50 female subjects when observing illustrations of pipes and purses. Males prefer pipes, C. R. 5.7; females prefer purses, C. R. 1.9.*

* Since women purchase approximately 80% of the household commodities, we might conclude that their interest would be more equally divided between their own and male interests.

Excursions are anticipatory and hence are for the purpose of orientation. Whether consciously or unconsciously, each eye movement accepts as its main purpose the task of adjusting the line of regard in such a way that the individual may see more clearly and organize more adequately the content under observation.

BASIC EYE MOVEMENTS AND TENDENCIES

The study of ocular patterns is significant first because it suggests that one type of performance is preferred over another; and secondly it raises the question whether a certain type of behavior is due to physical stimuli, the physiological organization or the nervous system, or the habitual mode of behavior or a combination of the three.

To discover whether the eyes themselves tend to follow a consistent pattern or whether they move at random without rhyme or reason has been the major objective in all of the research studies. In attempting to discover basic eye movement tendencies as revealed by ocular patterns, solutions to numerous questions have been sought.

Based on research, answers to the following questions have been attempted and reported in scientific and trade journals.

When observing a symmetrical field, where do the first, second, and subsequent eye fixations fall? Has the eye a preference for the right or left, top or bottom of a page? Does the eye prefer horizontal to vertical movements? How many fixations does the eye make in succession before it changes its course of direction? In what proportion do people employ foveal and peripheral vision?

What is the relative attention value of size? What role does isolation or white space play in advertising? Is the center position preferred over the outside space of a given field? Where should headlines in advertisements be placed to be most readily seen? What attentional value has color over black and white?

Do ocular patterns differentiate between good and poor learners? Are ocular patterns indices of intellectual ability? Are the ocular patterns of children radically different from those of adults? Can the methods the learner employs be discovered by this technique? What role do pictures play when competing with words as ideas when judged by the ocular performance of the observer?

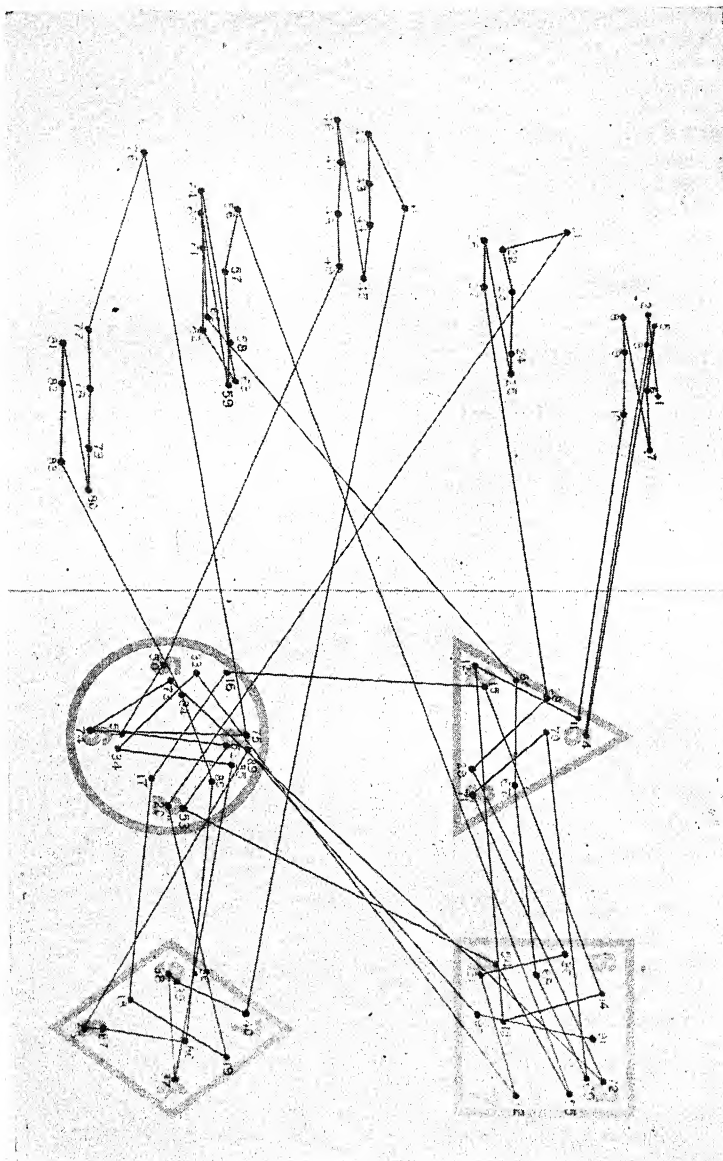


Fig. 7. Ocular pattern of subject 10 attempting to answer five questions.
All questions were answered correctly.

Can good and poor gunners be differentiated by analyzing ocular performance? What relation exists between ocular patterns and art appreciation? Are aptitudes of individuals revealed by their ocular performance under specified conditions? Can this technique be practically applied in crime detection? Do characteristic ocular patterns reveal personality traits (extroversion, introversion, etc.)?

Even though thousands of ocular patterns have been recorded and analyzed, it is evident that we have barely tapped the vast reservoir of information yet to be revealed by our orbits in motion.

OCULAR PATTERNS AND THEIR IMPLICATIONS

If the common purpose of ocular performance is to gain for the individual such orientation as will be best to accomplish his purpose, then each fixation implies selection, discrimination, judgment and organization. Successive fixations imply a type of exploration or detailed examination which generally terminate in a more unified or meaningful whole.

Attention: From the standpoint of the observer, the purpose of attention is to see more clearly and to understand more fully that which is being seen. Attention may be determined by the nature of a physical field, the interest of the observer at the moment, the habits resulting from past experience, or a combination thereof. Eyes in motion tell a story in the language of ocular patterns. The location, sequence, and duration of fixations constitute units of protensity (time), while distance and direction represent measures of extensity (space). Both represent a type of dimension, which may be ascertained by means of laboratory instruments.

Since attention is an important aspect of art advertising and learning, it is likely that ocular photography, properly employed, may reveal what catches and holds attention. Findings obtained by means of this technique and practically applied, may aid artists, educators, advertisers, and editorial writers to attach relative attention values to respective areas and condition of observation.

Interest: To appraise or announce our own interests is in

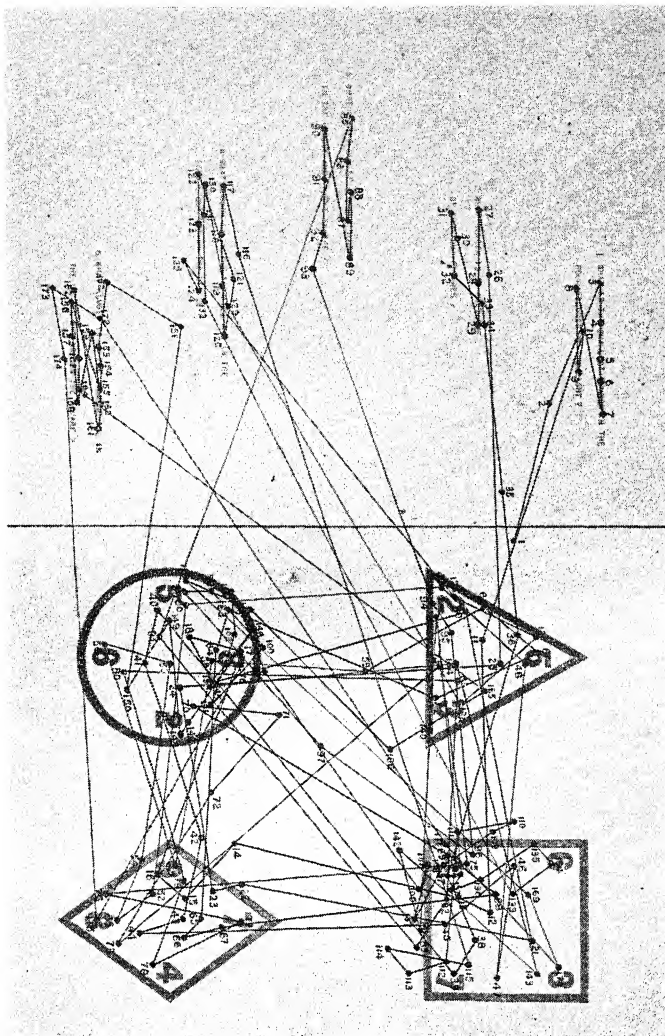


Fig. 8. Subject (15) answered 2 questions correctly, and made approximately twice as many fixations and traveled more than twice as far per unit of achievement as subject 10, his superior competitor.

most cases impractical and unsound. Many of our declared preferences are supported by rationalization or a type of justification not truly representative of our unconscious tendencies. Ocular photography may be the psychological approach to this important problem of obtaining valid first hand information about the interests and inclinations of an individual.

Since interest is determined by previous experience in terms of information, a degree of familiarity, a sense of satisfaction or a feeling of worth seems to be inherent, and an active effort to respond to respective situations is established.

The degree to which these interests may be discovered will depend largely on the adequacy of the design of the experiment itself. Properly obtained, such patterns constitute a graphic representation of human inclinations and desires, and may yield much valuable information to the teacher, parent and vocational counselor alike.

Learning: As evaluated by means of ocular photography reveals many of the mental processes involved. The ocular patterns may indicate the methods employed by the learner. Organization and differentiation are disclosed by the direction of the eye path and hence are indicators of relationships which exist or which, according to the learner, should exist. The reconstruction of a complex and obscure disordered whole is somehow managed by proper inspection, selection, rejection and combination of elements.

Although the adequacy of the construction depends on the capacity of the learner and his previous experience as well as on the inherent or coherent organization of the subject matter, it nevertheless finds expression in an ocular pattern, unique for the learner himself.

If the level of intelligence is ascertained by the behavior of an individual, it would seem logical that ocular patterns should within reason serve as an index of such ability.

Methods: If learning is a product of perceived relations, then it may be profitable for educators to inquire wherein the scale of observation the learner now stands. A redistribution of effort and a modified presentation of the form and quantity of the sub-

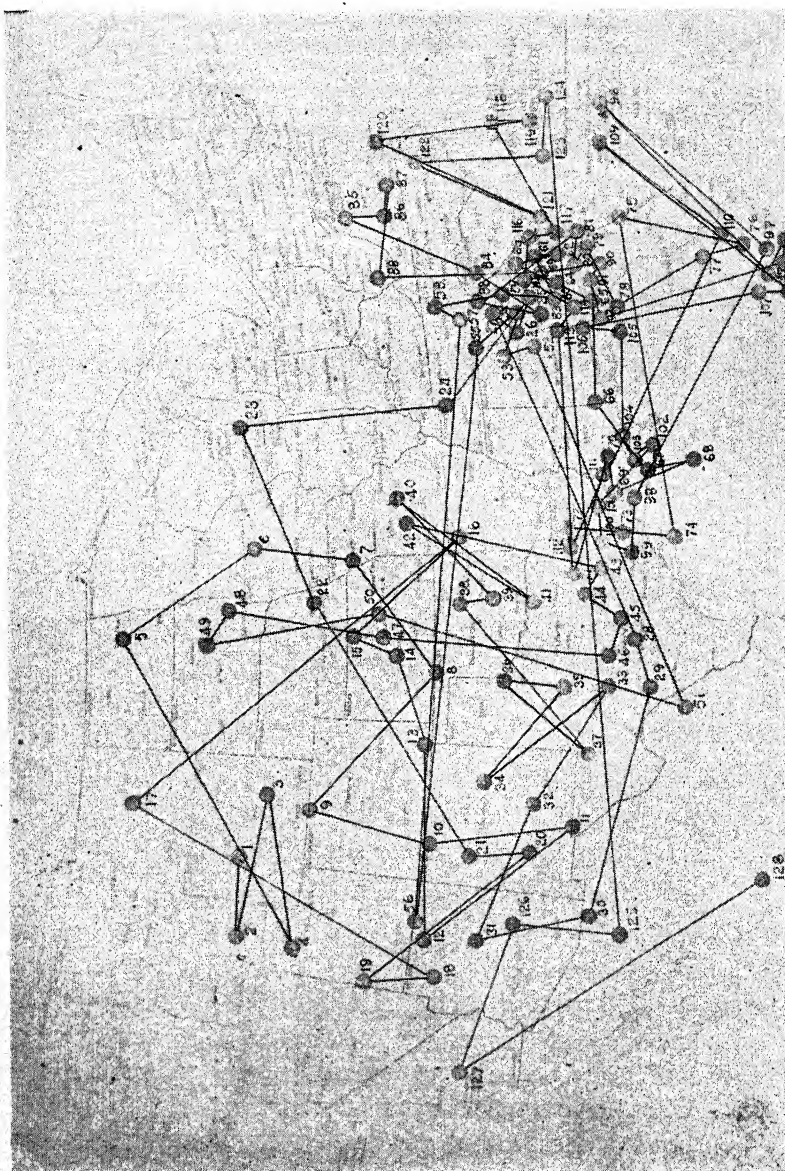


Fig. 9. Sixth grade student requested to locate capital of Georgia.
(Inferior student.)

ject matter may constitute essential conditions for increasing learning efficiency as measured by this technique.

Superior learners, whether due to nature or nurture, or a combination of the two, more than likely employ methods superior to their inferior competitors. If the above is true, then the ability to organize becomes the criterion of the possibility of reproduction later.

Organization as expressed in ocular performance is no longer an abstract idea. It has meaning in terms of methods and procedures employed by the learner in gaining the contents necessary for future use. For after all, what the individual learns is his own creation and will stand or fall as a result of its construction. To see relations and to reveal them in a graphic record boards on the subject of how we think or how we should think.

Indecision, confusion and bewilderment no doubt are responsible for some of the erratic patterns obtained from learners. The learner with adequate insight may reveal an ocular pattern quite different from one employing a trial and error procedure. A large number of random movements in areas of least importance may be an indication of waste and hence misapplied effort. Too often the criterion of efficient learning has been that of achievement, rather than an evaluation of the process employed by the learner. A perfect product may have been attained by an inefficient procedure. (See Figure 9.)

Affective Experience: In art objects, designs of colors are responsible for a certain type of experience of the observer, it is likely that the ocular pattern will be indicative of such experience? Aesthetical appreciation may be stimulated by large areas, straight or curved lines, balance or proportion, but regardless of such determiners, the eyes speak in terms of dimensions of time and space and thereby may reveal a certain interest value or satisfaction.

Based on such assumption it is likely that the ocular performance may reveal the character of the art object as expressed in color and design and hence serve as a criterion of the adequacy of the art presented.



Fig. 10. Sixth grade student requested to locate the capital of Georgia.
(Superior student.)

Foveal and Peripheral Vision: Throughout all the possible psychological implications of ocular performance when related to learning, looking at pictures, reading, or solving problems is found that of the relative function of foveal and peripheral vision.

If both foveal and peripheral vision have a place in making the necessary adjustments in education and industry, then it becomes the duty of the scientist to investigate the relative function of each to determine when and to what degree each should be employed to be most efficient.

Because of the emphasis on speed in modern civilization the visual mental adjustment employs peripheral vision and as a consequence accuracy is replaced by impressions, inadequate perception, poor memory and bad mental habits. It may even lead to disordered conduct, a neurotic temperament and a warped personality.

Based on the native capacity of an individual there is a very definite rate and limit of speed of perception and cognition. Whenever the normal speed of the subject is exceeded, he is penalized in terms of inadequate discrimination and is robbed of self-confidence which is essential in the mastery of a situation.

Educators and industrial engineers alike may determine on the basis of ocular performance how to present subject matter and how to organize procedures in modern industry.

Aptitude: If the relative function of peripheral and foveal vision is essential in effective and efficient everyday living, it is altogether likely that the type of ocular performance employed by the individual is indicative of his nature capacity or aptitude. It is likely that the ocular patterns of taxicab drivers, athletes, and salesmen is materially different from those of mechanics, scholars, or artists. If so, the ocular performance properly analyzed may constitute a valid criterion for the selecting of a vocation and training for an occupation properly adapted to the abilities of the individual.

Efficiency: With the ever-increasing complexity of the demands of the social order, manifested by the multiplication of books and periodicals, the profusion of advertisements in local

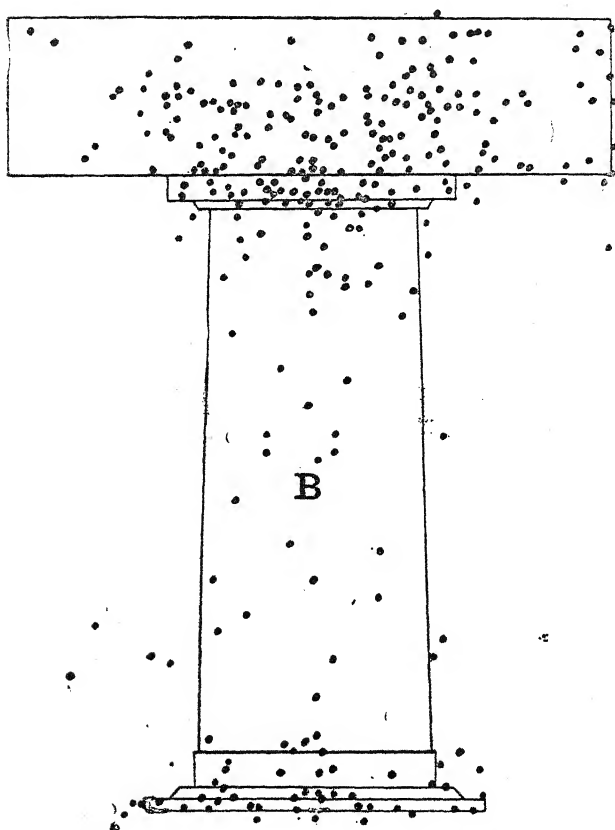


Fig. 11. Fixation concentration of subjects observing a balanced design.

and national publications, and the increasing number of fast-moving vehicles and machines, visual efficiency is becoming more and more essential. It is for such reasons that a scientific evaluation of how we see and how we should see is a matter of chief concern to everyone.

Whatever the human mind learns for purposes of adjustment is, as a rule, inadequate, inaccurate and inefficient unless properly directed in the initial and subsequent stages of acquisition. This applies to playing a piano, operating a press drill, running a typewriter, or playing golf, alike. If directed training is practical and profitable in industry and play, it would seem

feasible that ocular efficiency could be increased if the motor processes were known.

SUMMARY AND CONCLUSION

This paper has been an attempt to present a brief description of a new approach to the study of human behavior. By so doing, the author has probably placed undue emphasis on ocular photography as a scientific technique. This, however, does not imply that other methods of investigation are less important or inadequate.

Quite to the contrary, the author believes that all scientific methods employed in psychological research should supplement each other and ultimately corroborate their final conclusions. Scientific Psychology is today displacing the unsound practices known by their labels as astrology, phrenology, physiognomy, graphology, palmistry, numerology, telepathy, etc., etc., by an objective method of appraising man and his behavior. Psychology is no longer a name applied to wishful thinking and make-believe; it is a science because it employs methods of observation common to well established sciences. To describe, predict, and control is the aim of this new science.

Psychology, although only an infant in the family of sciences, is destined to make unprecedented progress due to the fact it has at its disposal, such highly developed sciences as physics, chemistry, biology, engineering.

In order to keep pace with the complicated and fast-moving social and industrial order, it is imperative that psychology utilize new and more highly developed mechanical devices. These new instruments of precision will yield new psychological concepts and units of measurements, essential in obtaining the goal it has set for itself, namely, that of discovering the principles of behavior.

Ocular photograph as one of these devices is destined to have far-reaching effects, mainly because it measures the most intricate of human adjustments and thus reveals certain psychological processes involved. With the photo-electric camera now in preparation, the elimination of sensitized film and laborious

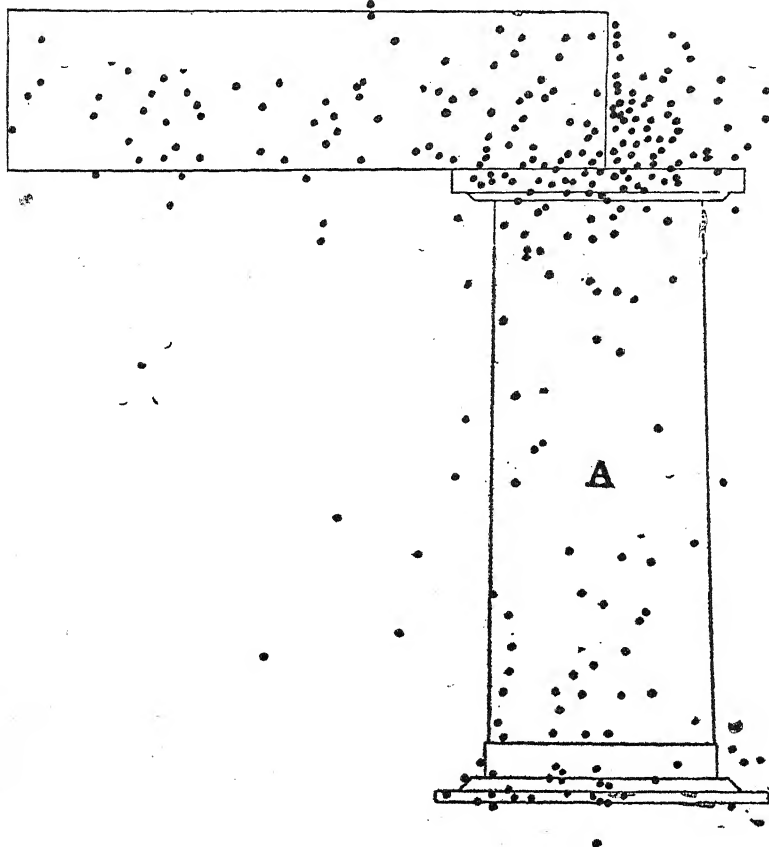


Fig. 12. Fixation concentration of subjects observing an unbalanced design.

statistical computations should constitute a major advancement in the field of psychological instrumentation.

Only by employing instruments of precision and by pooling our concepts based on each measurement, will we be able to project our thinking to establish psychology as one of the major sciences, a place it rightfully deserves.

THE ACOUSTIC CHARACTERISTICS OF THE EAR

ERNEST GLEN WEVER

Princeton University

The auditory mechanism may be considered as made up of two principal segments, one mechanical, by which acoustic stimuli are received and conducted inward to the sensory receptor cells of the cochlea, and the other neural, by which impulses are excited in the auditory nerve and relayed to the higher nervous centers. The discussion that follows is concerned only with the first segment, and to a large extent is based upon experiments carried out in recent years by the writer and his colleagues.

In the analysis of the operation of the peripheral auditory mechanism, three principal problems have arisen. They are (I) the operation of the middle ear apparatus as a mechanical transformer, (II) the functions of the tympanic muscles, and (III) the nature and locus of distortion in the ear. Though these problems are by no means separate, but interlock in complex ways, they will be treated generally in the order named.

I. THE ACTION OF THE MIDDLE EAR APPARATUS

Within the tympanic cavity are the eardrum, the three auditory ossicles—malleus, incus, and stapes, and the membrane of the oval window, together with the two tympanic muscles and the ligaments which unite and suspend these structures. The muscles will be considered in the following section. The remaining parts comprise the direct transmission system which serves as the pathway of conduction of vibrations from the air contained in the external auditory meatus to the fluid contents of the cochlea.

Since 1869, when Helmholtz (1, 2) first presented his theory of the middle ear, a persistent problem has been the operation of this system as a mechanical transformer.

The function of a mechanical transformer is to increase the efficiency of transmission of energy from one system to another. When acoustic energy is to be transferred directly from one medium like the air to a second medium of different density and elasticity like the fluid of the inner ear, the efficiency of transfer is poor, for most of the energy that strikes the junction is reflected and fails to enter the second medium. The degree of transmission between two mediums depends upon the ratio of their acoustic resistances, which in turn depends upon their densities and elasticities.

Unfortunately, the density and elasticity of the cochlear fluid are unknown. Since this fluid is confined in a narrow space and is in close physical relation to other anatomical parts it cannot act like a free fluid. Very likely its characteristics vary somewhat with frequency. However, for the purposes of the present discussion some assumption is necessary, and we shall probably not be entirely outside the bounds of reality if we treat the cochlear fluid as if it were an extended medium with the properties of sea water.

A calculation of the efficiency of transmission from air to cochlear fluid, on the basis of this assumption, gives a value of 0.001, or 1/10th of 1 per cent, which is a very low figure indeed. In its consideration we discover the reason that no animal has its inner ear exposed directly to the air, but interposes a middle ear apparatus to make a better match with the medium.

As Helmholtz formulated the problem of the reception of aerial vibrations, it is one in which the small forces exerted by the air particles must be brought to bear upon the inner ear fluid with increased mechanical advantage, analogous to the situation in which a man sets about with mechanical devices like levers and compound pulleys to move a heavy weight.

The ideal transformer ratio for the ear is difficult to estimate for the reason just given, but on the assumption which has been made this ratio comes to about 63 to 1, which is obtained

as the square root of the quotient of the acoustic resistances of sea water and air.

Helmholtz suggested three means by which the desired transformer action may be achieved in the ear.

One is a lever system. According to Helmholtz's measurements of the auditory ossicles, the tip of the malleus is about 1.5 times as far from the axis of rotation of the ossicular system as the head of the stapes. The vibration communicated to the perilymph by the stapes will be increased in force, though at the expense of a corresponding decrease in amplitude.

A second form of mechanical advantage is hydraulic. The area of the tympanic membrane is, according to Helmholtz, some 15 to 20 times larger than the area of the oval window. Hence, the pressure communicated to the perilymph will be increased correspondingly over that effective at the drum membrane.

The third mechanical advantage outlined by Helmholtz was considered to arise from the conical form of the tympanic membrane and the manner of attachment of the malleus. The membrane is concave as seen from without, and the malleus is attached along the superior radius with its tip near the center of the cone. Helmholtz supposed that as the apex of the cone is moved in and out by the sound pressures the movement of the malleus is largely in a radial direction, and is significantly less in amplitude but greater in force. He compared the system with a horizontally stretched string whose weight communicates a comparatively great tension of the two anchor points. What is conceived here is evidently a kind of mechanical lever, but though Helmholtz asserted that it was the most important means of pressure reinforcement, he offered no numerical estimate of the action.

The above three suggestions will now be examined in detail.

1. *The ossicular lever system.* Helmholtz's suggestion of a simple lever system formed by the ossicles has been generally accepted. A recent discussion is that of Stuhlman (3), who worked with a large scale model. From his observations he concluded that the lever ratio between malleus and incus depended upon the nature of the coupling at the joint between

these ossicles. If the joint was locked the ratio of the lever arm of the malleus to that of the incus was 1.27 to 1. If the coupling was loose the ratio was about 2 to 1 for inward motion and 1 to 1 for outward motion. He regarded Helmholtz's figure of 1.5 to 1 as an acceptable overall value.

This procedure of inferring the lever ratio of the ossicular chain from an anatomical examination or work with models faces a well-nigh insuperable difficulty due to the complex nature of the structure. It is not possible to determine accurately the locations of the fulcrums, or even the points of application of the forces. It is necessary to make assumptions regarding the stiffness and "play" of the articulations, the relative tensions afforded by the suspensory ligaments, and the effects of the tympanic muscles. As these assumptions are arbitrary, the conclusions that can be drawn are equally so.

When so little is known, a simpler theory involving fewer assumptions seems preferable. It is here suggested that the ossicular system of the mammalian ear operates in a manner analogous to the columella of amphibians, reptiles, and birds, which is a simple rod connecting drum and oval window. The ossicles operate as if their joints were locked to form a rigid conductor. As will be shown later, this view agrees better with the results on auditory distortion than one that requires levers, articular movements, and loose and varying joints.

1. *The hydraulic action.* There is no reason to doubt the operation of the second principle mentioned above, afforded by the areal difference of drum and oval window. Helmholtz gave an estimate of the areal ratio as between 15 and 20, and in recent discussions the figure 20 is most often mentioned. Some of the measurements of drum and oval window yield a ratio as large as 30, and it is possible that there are large individual variations.

3. *The drum-lever principle.* The principle that Helmholtz regarded as the most important has received the least attention, perhaps because the other two have been considered sufficient. Let us first consider as a simple approach to the problem Helmholtz's example of a string held at both ends and displaced at the center. The mechanical advantage of the central force in

its action against the end supports will of course vary with the amount of displacement at the outset, that is, with the angle made by the string and a line joining the supports. If we make this angle 16° , which corresponds approximately to the angle between the axis of the manubrium of the malleus and the tympanic ring, the sine of this angle will represent the ratio between the applied force and the pull on the supports. The force against each support will be 3.6 times the applied force, which is a notable mechanical advantage. However, in the eardrum we have many radial fibers, not a single pair, and hence it is necessary to develop the problem further. Suppose instead of a single string we had used a bundle of 12 strings; if the same force were applied as before we should have a resultant tension of each string against the support of only $3.6/12$ or 0.3 of the impressed force. In this example, 12 is not simply a random figure, but is derived from the area of drum membrane covered by the manubrium, and hence expresses the proportion of radial fibers whose tensions are communicated to the malleus, according to the theory. The figure 0.3 thus indicates a loss of pressure rather than a gain. This development of Helmholtz's hypothesis is thus unfavorable to its acceptance.

It seems preferable to dispense with the idea of relative motion between drum and malleus, and to consider the drum membrane as itself sufficiently rigid and so firmly attached to the malleus as to transmit directly the pressures exerted by the air particles.

Our consideration of the drum membrane as a rigid surface is of course limited by the fact that its edges are fixed. What we must picture, therefore, is a central portion of the membrane whose points all move essentially in phase, and a bounding area that must exhibit a gradient of motion down to zero at the tympanic ring. The central, mobile portion of the membrane is thus equivalent to a simple piston of an area smaller than that of the whole membrane. This reduction of a fixed membrane to an "equivalent piston" is a common practice in acoustical engineering in dealing with the motions of telephone diaphragms, loud-speaker cones, and the like.

These considerations make meaningful the particular form of the eardrum. Its roughly conical form adds materially to the rigidity of its central portion. Therefore the effective area is considerably greater than would be true for a plane membrane of equal size. Loudspeaker diaphragms are usually stiffened in a similar manner.

The effective area of the drum can only be estimated; possibly it is around $\frac{3}{4}$ of the actual drum area. The effective area of the oval window is derived more simply, for it is the area of the footplate of the stapes, which covers all but a narrow margin of the membrane.

The preceding discussion suggests these conclusions regarding the operation of the middle ear apparatus. The drum may be represented as a rigid piston of perhaps $\frac{3}{4}$ the anatomical area. Its motions are transmitted directly to the ossicular chain, which itself operates like a columella, transmitting the vibrations to the oval window and the fluid of the inner ear. Of the three means proposed for achieving an efficient transfer of energy from air to cochlear fluid, but one remains assured, that arising from the difference in areas at the two ends of the chain. From a consideration of the effective areas it appears that a reasonable figure for the transformer ratio is about 18 to 1. This is less than a third the value assumed as ideal, yet it represents a substantial gain none the less. It gives 28 o/o of maximum transmission as against 0.1 per cent, or a power loss of only about 5.6 db instead of a loss of 30 db which would be suffered if there were no transformer action at all.

II. THE FUNCTIONS OF THE TYMPANIC MUSCLES

There are two tympanic muscles which by their contractions set up tensions between the ossicular chain and anchorages in the walls of the middle ear cavity. The larger, the tensor tympani muscle, is inserted by means of its tendon upon the manubrium of the malleus, and is directed medialward. The stapedius muscle is applied near the head of the stapes, and exerts tension in a posterior direction roughly at right angles to the axis of the stapes.

In past discussions, various roles have been conceived for these muscles. The following treatment is based upon experiments with cats in which the muscular actions have been simulated by exerting tension on a thread attached to the tendon, while observing the effects upon conduction by means of the electrical responses of the cochlea (5, 7).

1. *The tensor tympani muscle.* The effects of tension applied to the tensor tympani tendon are a complex function of the frequency and intensity of the sound that is being transmitted by the middle ear apparatus. Except for the extreme stimulus intensities, all tones suffer a considerable reduction in magnitude, though the degree of reduction varies according to frequency. Roughly speaking, the effects are greater for low-frequency tones than for high. More exactly, however, the variation with frequency appears to depend upon the fact that contraction of the muscle introduces damping and at the same time increases the stiffness of the conductive system. Owing to the change of stiffness the resonance characteristics of the system are altered, and the sensitivity is not only reduced but is shifted somewhat toward the high-frequency end of the tonal scale. The effect of this two-fold change is to depress the sensitivity most for frequencies just below a maximum of sensitivity or just above a minimum, whereas frequencies just above a maximum or just below a minimum are least affected.

Tones which are applied at intensities above ordinary levels show peculiar changes due to the particular form of the intensity-response function at high levels. The application of tension may bring about only slight changes, or may even produce initially an augmented response. This seemingly anomalous result appears because at high levels the cochlear response ceases to be a simple function of stimulus intensity: it approaches a maximum and then bends downward so that at the extreme levels an increase of stimulus intensity causes a reduction in response. Since this overloading effect takes place in the inner ear, it follows that tension on the tensor tympani tendon, which reduces transmission, has the effect of a reduction of stimulus intensity, and in this region causes an increase in response.

2. *The stapedius muscle.* Tension applied to the stapedius tendon produces results generally similar to those described for the tensor tympani. The most marked effects are for the low frequencies. For frequencies in the region of the maximum of sensitivity, which for the cat is about 3000~, the initial effect is an improvement in transmission, but this effect soon reaches a limit and for the higher degrees of tension passes over into a reduction. Frequencies above the region of greatest sensitivity suffer somewhat smaller reductions than the lower tones.

The tympanic muscles serve a protective function, in two senses. In the first place, they add significantly to the mechanical security of the transmission system. If both the tendons are severed the ossicular chain is extraordinarily fragile, and is often injured by even cautious manipulations. Indeed, it is difficult, after one tendon has been cut, to sever the second without doing some injury to the ossicular articulations or connected structures, due to the ease with which the chain may now be displaced in any direction.

The principal function of the tympanic muscles no doubt is the protection of the inner ear against injury by excessive sounds. We have observed repeatedly that when an animal is curarized to the point where these muscles become inoperative, it is necessary to exercise unusual caution in the application of stimuli, for cochlear injury of serious degree will result from even brief applications of sounds that normally may be borne without risk.

In their protective role the two muscles operate synergetically. Although there are minor variations in their effects, both serve to reduce sound transmission. Of the two, the stapedius muscle is the more effective despite its smaller size: a given degree of tension on its tendon reduces transmission about 10 times as much as the same tension on the tensor tympani tendon. This difference doubtless arises from the difference in the direction of action of the two muscles. With the stapedius muscle the force is applied roughly at right angles to the direction of ossicular vibration, while with the tensor tympani it is more in line with the vibration.

These muscles have a peculiar arrangement of their fibers which enables them to exert a considerable tension without causing much actual displacement. Their fibers are all in parallel, so that every one contributes to the total force, yet the shortening is only that of a single fiber, and hence is a minimum. The latter characteristic is desirable so that serious modifications of the form and position of the ossicular chain may be avoided. To some extent the two muscles operate in contrary directions, which further reduces such modifications.

The appearance of displacements or realignments of the ossicles as a result of contraction of the muscles is not contradictory to the hypothesis presented earlier that the joints are locked so far as audio-frequency movements are concerned.

III. THE NATURE AND LOCUS OF DISTORTION

If the action of any acoustic mechanism fails to maintain a simple linear relation to the acoustic forces impressed upon it, the mechanism then is said to suffer non-linear distortion. The ear is such a mechanism. Tones which are physically pure are heard as complex when their intensities are sufficiently great. This complexity is observed as the emergence of overtones, and when two or more stimulus frequencies are simultaneously present, as combination tones as well.

That the distortion which produces the overtones and combination tones has its seat in the mechanism of the ear is shown by studies of the electrical potentials of the cochlea, for the additional frequencies appear in a wave analysis of these responses.

Observations have been made on various animal ears. The following were carried out on the cat (6). The ear was stimulated with a 1000~ tone of a high degree of purity at various intensities, and the resulting responses recorded with a wave analyzer. For the lowest intensities only a 1000~ response was obtained, but as the intensity was increased gradually it became possible to analyze out of the response wave a faint 2000~ component, the first overtone or second partial, in addition to the 1000~ fundamental. A further increase of intensity brought in the third partial of 3000~, and at the same time raised the sec-

ond partial to a much higher level. A still further increase brought in components of yet higher orders: 4000~, 5000~, and so on up to 16,000~ (which was the limit of operation of the apparatus).

The various partials all behave in a similar manner, in that after they become noticeable they rise rapidly according to a power function of stimulus intensity, and then finally depart from this simple course by attaining a maximum and bending downward. For the fundamental the power exponent approaches unity, which means that the function is linear with respect to stimulus intensity. For all the higher components the exponent is greater than unity, which means that the magnitude of the component rises more rapidly than the fundamental, until overloading sets in. For the earlier components it may be stated that the slope is greater the higher the order of the component. but for the upper orders any relation of this kind is obscured by irregularities due to the small magnitudes observed and the early appearance of overloading.

In general, the magnitudes of the components vary according to their serial order, but this relation is cut across by the fact that overloading appears at different levels. Overloading usually appears earlier for even-numbered partials than for their immediately succeeding odd-numbered partials, hence these odd members become relatively more prominent at the higher levels. Sometimes further irregularities are shown, especially by the even-numbered partials, which in overloading reach a maximum and fall, and soon thereafter show a secondary rise.

The total harmonic content of the response varies progressively with the stimulus intensity. For a moderately high intensity it reaches about 10 per cent of the magnitude of the fundamental, while it may exceed 50 per cent for intensities near the limit that the ear can withstand without damage.

When two pure tones are applied simultaneously, and raised to sufficient levels, there are present not only the two series of overtones belonging to the two stimuli, but also a series of combination tones which represent their interaction products (4, 11). These combination tones are present in great numbers, and in-

deed appear to be limited mainly by sensitivity factors within the ear and in the apparatus used in their observation. The frequencies of the combination tones represent the sums and differences of the frequencies of the stimulus tones and their simple multiples; or, in algebraic terms, all absolute values of $(mh \pm nl)$, where h, l are the stimulus frequencies and m, n are any simple integers.

For example, when the primary frequencies of 1000~ and 2800~ are introduced into the ear, the cochlear response contains 1800~, which is the first order difference tone $h - l$, or $(2800 - 1000)$; 3800~, which is the first order summation tone $h + l$, $(2800 + 1000)$; 4600~, which is the second order difference tone $2h - l$, or $(2 \times 2800 - 1000)$; and so on. With no attempt to exhaust the possibilities we have recorded as many as 40 overtones and combination tones for a single pair of stimuli. When as many as three stimuli are applied, the resulting pattern is almost indescribably complex.

Though the combination tone pattern is numerically complex, it seems to follow definite laws. The magnitude of a component is a function of the intensity of its generating stimuli, plus an independent aural sensitivity factor. The specific functional relation between a combination tone and one of its generators depends upon the order in which the generator enters into its formation. This relation is most simply stated by the formula $f(mh + nl) \propto P_h^m P_l^n$, where the expression on the left indicates the magnitude of the combination tone concerned, and P_h and P_l represent the intensities of the two stimuli raised to the powers indicated by the integers m and n . This formula has been verified for components of lower orders, within the limits where overloading enters. A test with respect to the higher order components is prevented by their small magnitudes.

The above relation signifies that the first order difference tone, whose frequency is the difference in frequencies of the stimuli, will vary in magnitude as the product of the intensities of the two stimuli. A component of higher order, like the second order difference tone given in the example above as 4600~, produced $2h - l$, will vary as the square of h and linearly as l ,

or altogether as the product of b^2l . These relations show why in ordinary listening the combination tones become more noticeable when the fundamental tones are strong, until limits are reached where overloading enters.

The transformation theory. The nature of distortion, and the relations between both overtones and combination tones and their stimuli, are expressed in physical and mathematical terms in the transformation theory, first suggested by Helmholtz. This theory considers the nonlinear distortion as due to the fact that the displacements produced in the ear by impressed vibratory forces are not simply proportional to these forces, but are at the same time proportional to their square, cube, fourth power, and so on. If the reaction to a single tone follows such a power series, then it is easy to show by a trigonometric expansion that a harmonic series arises, and likewise that two tones give a harmonic series for each and a combination tone series. Moreover, the coefficients of the various terms of the expansion indicate, at least roughly, the relative magnitudes of the several resultants.

The locus of distortion. Helmholtz supposed that the locus of the transformation process was the middle ear. He considered the drum and ossicles especially as contributing to the distortion.

The drum, like any stretched membrane, ought to operate in non-linear fashion when its displacements become large. Moreover, the drum is unsymmetrical in form, and its radial fibers, he thought, ought to be stretched more by an inward movement than by an outward movement.

The ossicles, according to Helmholtz, ought to give distortion because their joints are loose, and consequently the motion which they transmit is not a faithful copy of the drum motion. Especially important here is the joint between malleus and incus, which he compared with a cog mechanism. When the malleus moves inward it carries the incus with it, but when it moves outward the joint surfaces separate and the incus lags behind.

Helmholtz's suggestions regarding the site of aural distortion have been accepted generally, but until recently there has been little definite evidence on the problem. Helmholtz's infer-

ences were drawn from a consideration of the anatomical structure, and most of the later discussions have had no further basis. Several investigations have been carried out with mechanical models, but while these serve to make the speculations graphic, they cannot be regarded as supplying discriminating evidence, as they necessarily are constructed according to the assumptions that ought to be under test.

A few observations have been made on the ears of cadavers with static or slow displacements of the drum and measurements of the resulting movements of the stapes, but the results are unsuitable both because the measurements have been inexact and because in the effort to obtain discernible displacements the parts have been subjected to forces far exceeding any encountered in normal hearing.

By use of the electrical responses of the cochlea a new attack has been made on this problem (8). The evidence obtained by this method agrees with the Helmholtz theory to the extent that distortion appears in the mechanism of the ear, but it does not agree with this view regarding the role of the middle ear. Rather, the inner ear is indicated as the chief site of the distortion process.

The part played by the middle ear in auditory distortion may be determined by comparing the distortion pattern obtained for the intact ear with that obtained when the middle ear has been eliminated surgically.

In one series of experiments the pattern of overtones was observed with and without the more peripheral portion of the middle ear. After measurements had been made on the intact ear, the ossicular chain was separated at the joint between incus and stapes, and stimulation was carried out by applying a mechanical vibrator to the head of the stapes. The operation eliminated the drum and the two larger ossicles, and also the tensor tympani muscle. In a further experiment the tendon of the stapedius muscle was separated in addition.

The results showed no essential differences between the distortion patterns for normal and operated ears. The same overtone components were present, in about the same degree.

In a second series of experiments (9) a similar procedure

was followed in a study of the combination tones. They too presented a high degree of similarity with aerial sounds delivered to the intact ear and with vibratory stimuli applied to the stapes after elimination of the more peripheral structures. However, in some instances the two patterns were not identical, and it was concluded that though the inner ear is the chief site of distortion, the middle ear under some conditions may contribute to the total pattern.

The above observations were made on the ears of animals under deep anesthesia. The presumption is that in the waking state, with increased tonus of the middle ear muscles and hence a higher degree of rigidity of the conductive mechanism, the distortion in the middle ear should be even less than under the experimental conditions. Additional evidence on this point is derived from experiments on the action of the tympanic muscles.

In animals whose middle ear structures were intact, the effects of the muscles upon distortion were studied in a number of ways (5, 8). These included (1) cutting the tendons, (2) denervating the muscles, (3) eliciting reflex contractions of the muscles by stimulating the pinna, and (4) exerting artificial tension on the tendons.

The first two procedures had no noticeable effect upon the distortion pattern. The third was not carried out very systematically, as the reflex contractions are variable under these conditions. A reduction of the distortion pattern was observed, but no more than may be accounted for by the fact that the entire response was brought to a lower level.

The fourth procedure caused no change in the distortion pattern when carried out for the tensor tympani tendon, but it did so for the stapedius (7). The application of tension to the stapedius tendon either increased or decreased the total distortion depending on the degree of tension. For any particular tension, the different overtones were affected in different ways. The animals showed individual variations, but generally speaking the smaller tensions gave only moderate increases or decreases in the overtones, while the larger tensions produced a considerable rise or, sometimes, had no further effect.

These irregularities, and especially the rapid changes of distortion at times observed for a relatively small alteration in tensional load, suggest that tension on the stapedius tendon, especially if it is great, may upset the normal action of the middle ear.

A further procedure which is of interest in relation to the problem of distortion is an increase in the air pressure of the middle ear cavity (12, 13). Such a pressure increase affects considerably the mechanical state of the transmission system, for it presses the drum outwards and pushes the stapes inward in the oval window. The membrane displacements necessarily alter the mutual relations of the ossicles. These changes in the middle ear apparatus have a marked effect on sound conduction. However, when the loss of sensitivity is compensated for by raising the level of stimulation, the distortion remains similar in amount and character to that found under normal conditions.

The above considerations lead us to the inner ear as the locus of the distortion process. The specific locus here is probably the hair cells of the organ of Corti, the final elements of the auditory receptor process which appear to be responsible also for the appearance of other important developments, such as overloading, interference, and generation of the cochlear potentials (10, 14).

The above results show that the middle ear mechanism is well adapted to its functions of sound conduction. Under usual conditions it operates with high fidelity. Some degree of distortion can not be denied, especially when the stimuli are strong, yet this distortion is small, and ordinarily negligible, in relation to that which arises later on in the cochlea. Certain measures, like large tensions on the stapedius tendon, may introduce considerable non-linearity, probably because the normal positions of the parts are altered. Other mechanical changes, however, like tension on the tensor tympani tendon and alterations of air pressure in the middle ear cavity, have little effect.

These observations support the view expressed earlier that the transmission of sound from drum to inner ear is essentially simple in character, like that of a columella. If there were relative movements of the ossicular joints, or looseness, or compli-

cated lever actions, we should almost certainly find distortion in high degree, for "play" in the connections of the parts would be greater for the larger amplitudes of movement. As distortion is small, the movements are closely representative of the acting forces; and this condition is most simply attained if the entire drum and ossicular structure operates as a rigid conductor.

REFERENCES

1. HELMHOLTZ, H. Die Mechanik der Gehörknöchelchen und des Trommelfells, *Pflüg. Arch. Physiol.*, 1869, 1, 1-60.
2. HELMHOLTZ, H. *On the sensations of tone*, 2nd Eng. ed, tr. by A. J. Ellis, London: Longmans, Green, and Co., 1885, pp. 130 ff., 158.
3. STUHLMAN, O., JR. The nonlinear transmission characteristics of the auditory ossicles. *J. Acoust. Soc. Amer.*, 1937, 9, 119-128.
4. WEVER, E. G. The designation of combination tones. *Psychol. Rev.*, 1941, 48, 93-104.
5. WEVER, E. G., and BRAY, C. W. The tensor tympani muscle and its relation to sound conduction. *Ann. Otol., etc., St. Louis*, 1937, 46, 947-961.
6. WEVER, E. G., and BRAY, C. W. Distortion in the ear as shown by the electrical responses of the cochlea. *J. Acoust. Soc. Amer.*, 1938, 9, 227-233.
7. WEVER, E. G., and BRAY, C. W. The stapedius muscle in relation to sound conduction. *J. Exper. Psychol.*, 1942, 31, 35-43.
8. WEVER, E. G., BRAY, C. W., and LAWRENCE, M. The locus of distortion in the ear. *J. Acoust. Soc. Amer.*, 1940, 11, 427-433.
9. WEVER, E. G., BRAY, C. W., and LAWRENCE, M. The origin of combination tones. *J. Exper. Psychol.*, 1940, 27, 217-226.
10. WEVER, E. G., BRAY, C. W., and LAWRENCE, M. The interference of tones in the cochlea. *J. Acoust. Soc. Amer.*, 1940, 12, 268-280.
11. WEVER, E. G., BRAY, C. W., and LAWRENCE, M. A quantitative study of combination tones. *J. Exper. Psychol.*, 1940, 27, 469-496.
12. WEVER, E. G., BRAY, C. W., and LAWRENCE, M. The effect of middle ear pressure upon distortion. *J. Acoust. Soc. Amer.*, 1941, 13, 182-187.
13. WEVER, E. G., BRAY, C. W., and LAWRENCE, M. The effects of pressure in the middle ear. *J. Exper. Psychol.*, 1942, 30, 40-52.
14. WEVER, E. G., and LAWRENCE, M. Tonal interference in relation to cochlear injury. *J. Exper. Psychol.*, 1941, 29, 283-295.

THE PSYCHOPHYSIOLOGY OF SET

R. C. DAVIS

Indiana University

An investigator in the field of physiological psychology has the choice of two kinds of procedure. On the one hand he may take as his starting point some physiological processes, neural, endocrine, circulatory, etc., and trace in them the effects of stimulation of the organism. He may on the other hand choose to begin with psychological processes, identified by analysis of behavior, and search for whatever physiological processes may comprise them. Whatever hopes there may be for ultimate junction of the two approaches, clarity requires that an investigator know from which direction he is proceeding, for it would hardly be proper to assume that the physiological concepts employed in one approach are congruent with the psychological employed in the other.

It is plain that a discussion of the psychophysiology of set begins at the psychological end of the bridge, with a concept defined on the basis of organismic action; but specifically, how defined? The investigator certainly has a wide choice of definitions of this term, as of other psychological concepts. A recent article by Gibson (16) is an examination of the meanings attached to the word and its correlatives in various accounts. Gibson concludes that "no common meaning can be discerned". Literally taken, this is perhaps an over-statement. For a definition of sufficient generality (*e.g.*, "a set is a determining tendency in the organism, subject to change under certain conditions") would probably apply to all usages. Although this would include

enough territory, it certainly would not exclude enough to satisfy most. It is in their subdivision of this broad territory that accounts differ and conflict. (This is possibly what Gibson meant.)

For the purpose of handling experimental results, a very broad definition of the term is linguistically inconvenient; too many situations must be investigated before the term can be used. A narrower concept referring to a particular experimental situation is easier to use, but, of course, merely postpones the question of generality. A large number of experimental situations, such as the delayed response, the conditioned response, the association experiment, the transfer experiment, and others described by Gibson would be available as defining situations. Among them some seem to suggest the presence of an active process of rather definite temporal course; others a more passive, potential condition. Whether or not such a distinction will prove valid upon further examination, it is the active process which promises greater success to psychophysiological study. Two such phenomena, the fore-period process in the reaction time experiment, and the process underlying time error in stimulus judgment by the constant method, suggest themselves, since there is a considerable body of psychological information about them. It is proposed, therefore, that for the present purpose "set" be considered as defined by those two experimental situations.

In the reaction time experiment, more specifically, the strength of the set is defined as varying with the complement of the reaction time as in the equation $S = k(a-t)$, where S is set, t is reaction time, and k and a , positive constants. (In some experimental work a reciprocal function has been used. The alternatives are really indistinguishable with the present degree of experimental refinement.) In treating judgments of stimulus magnitude, it is customary to separate the constant and variable errors (or thresholds) mathematically and to propose a different theory for the explanation of each. In the process, the original data in the form of proportions of judgments in the several categories are transformed into an abscissa value subtending an ogive or a similar curve. Again, in the present state of knowledge at least,

it may be as well worked with a simpler formulation, and to consider the set as defined directly by the proportion of judgments in a given category (the variable error or threshold and comparison weight being unchanged). For a two-category experiment, our definition would be $S = k(p-.50)$, where p is the proportion of "greater" judgments and k is positive. Since responses in the judgment of stimulus magnitude are qualitatively different, we must, at the outset anyway, recognize an equal number of qualitatively different sets, defined in similar fashion. Even in the simple form of the reaction-time experiment alternative sets are undoubtedly operating in spite of the experimenter's intention. In the stimulus judgment situation, as to a lesser extent in the disjunctive reaction experiment, several alternative sets can be traced.

Whether these concepts are the "right" ones to use will be decided, of course, by the measure of success which attend their use. Other functions might work better, or it might be advantageous to sub-divide these concepts. An experimental distinction might be made, for example, between sensorial and motor dispositions, or between their modern counterparts, "expectancy" and "intention" as Gibson (16) and Hilgard (17, 18) have proposed. Present physiological data, however, are not ample enough to decide the point.

With the concept identified, the program proposed for the discussion is first to survey the results of psychological experiments on set, so defined; that is, to see what variables set is related to, and how it is related. Then we shall be in a position to explore what is known of various physiological processes to see whether they display similar functions of the same variables. This is the method of investigation which has been urged by Mowrer (28).

Aside from the proposed definitional characteristics there are quite a number of important facts known about set. In the reaction time experiment, for example, we may observe a number of time relationships. The well-known results of Breitwieser (2) and Woodrow (37) indicate that the set rises to a maximum and then recedes. Mowrer (28) has lately shown that the rise be-

gins almost immediately after the ready signal and continues regularly up to the maximum when it is replaced by a gradual decline. Taken together, the results of Woodrow and Mowrer (and others) show that the maximum point of the set can be shifted widely by instructions and practice, and the whole process be varied considerably in the duration. The maximum set condition occurs at the interval to which the subject is accustomed. From Woodrow's results we may further conclude that maxima occurring at different times are unequal, the greatest appearing when the subject is accustomed to a fore-period of about two seconds.

A further modification of the time function of set is seen in the facts of facilitation and inhibition summarized by Woodworth (38). The effect of an additional stimulus delivered simultaneously with the reaction signal would scarcely fall within the proposed definition of set, although it might turn out to be a limiting case of the same process. When the additional stimulus precedes the reaction signal, we undoubtedly have an instance of set as here considered. Todd's (36) investigation showed that a shock preceding a visual reaction signal by less than a tenth of a second has a facilitating effect, *i.e.*, increases the set. At two-tenths of a second, on the other hand, the effect is reversed, and at a still larger interval it approaches zero. Whether the usual ready signal given with extremely short intervals would have the same cyclic effect has apparently not been ascertained.

The presence of a continuous or recurring ("incentive") stimulus prior to the response signal likewise modifies the set. A continuous background of sound, shortens the reaction time to light (Jenkins, 20; Knott, 22). Knowledge of results, and shock punishment for slow reactions both act as positive incentives (Johanson, 21). In the study by Jenkins (20) a shorter reaction time to cessation than to onset of stimulation was demonstrated.

Another influence upon simple reaction time is the factor of practice. With an entirely naive subject this may be substantial. To be sure, readiness derived from practice may be of a different sort from that investigated in the studies of time rela-

tions; but it seems worth while tentatively to include this characteristic as one of the functions of set.

The same may be said of the difference between the simple and the disjunctive reactions; but, if they may be handled by the same concept, we may say that the set in the simple reaction is considerably stronger than that in the disjunctive reaction. In fact, the various types of disjunctive and associate reactions provide a long series of progressively weaker sets,—or, alternatively, strong but inappropriate sets. To the series may be added the information lately contributed by Mowrer and his collaborators (29, 30) on reactions which are identical, but made, by instructions, to alternative stimuli. The experiments were reported for their bearing on the locus of set. Disregarding that point for the moment, it may be said that the results bear on the question of equivalence of sets. A set for a stimulus in one sense modality is shown to be equivalent to a set for another in the same modality (when the response is the same) but to be non-equivalent to a set for a stimulus in another sense modality at first. After a certain amount of practice, however, the set becomes generalized so as to allow as quick a response to one modality of stimulus as to another.

In the second experimental "set" situation, judgment of stimulus magnitudes, we find the time function as the most important feature beyond the definitional characteristic. As described originally by Köhler (23) (in somewhat different terms), a set for judging the second stimulus as less rises immediately after the first stimulus, reaches a maximum at about 2 seconds, recedes to zero, and then becomes progressively negative. Further explorations show a still later movement toward a positive phase. The set for judging the second stimulus as greater has, of course, a converse form. Such a time function of set appears under certain conditions at least in the comparison of stimuli in a number of modalities. The curve is rather suggestive of the facilitation-inhibition relationship in the reaction time experiment, and, indeed, of the similar effects Bowditch and Warren (3) long ago demonstrated for reflex action.

Investigators of the time error in recent years have been

concerned chiefly with the possibility of modifying this time function under varying conditions and with the significance of these modifications. Practice in judging magnitudes tends to produce a radically different time function (Needham, 31). Woodworth (38) has summarized a number of studies on the effects of interpolated stimuli of the same modality as those being judged. The general result in our terms is that a relatively strong interpolated stimulus increases the set for judging lighter and a weak one decreases it. The time at which a particular stimulus is interpolated seems to be important but in certain conditions stimuli rather remotely antecedent to either of those judged appear to have the same effect as an interpolated stimulus. Marchetti (27) and McClelland (26) have shown the effects of background conditions of the standard stimulus in comparisons of visual magnitude.

It is evident that we already have a wealth of facts about "set" in the two designated experimental situations, and we may proceed to an inquiry as to whether any known physiological processes display the properties so specified. Since the time relations are so important and definitely known, it may be desirable to search first for some sort of facilitating or inhibiting process which could vary with time in the specified manner. This means, of course, a process which reaches its full development some seconds after it is initiated by a stimulus. It is this slowness which raises the greatest difficulty, inasmuch as the better known processes of the central nervous system and the voluntary musculature (evidently the chief factors in the subsequent response) are faster in their action. It is not that sets are unique in this slowness. Every reaction which is slower in onset or development than the simple reflex raises the same problem. The question then, is, what parts of the organism operate with such a slowness, and how do they affect later responses?

The main possibilities may be set forth as follows:

1. There is some process within the brain which has a long time-constant, subject to systematic variation. Obviously any facilitative process would then take place in the brain.
2. There occurs a mutual interaction between brain and

muscle, each sustaining an excitation in the other. In this case the facilitation might take place at either level.

3. A persistent sense organ excitation or slow reflex adjustment of receptor accessories affects reception of the later stimulus.

4. The whole system later to be involved in the response is put into action and remains in a state of "sub-threshold" excitation. There would then be no special locus for the facilitating effect.

Some of these proposals clearly venture farther than others. While the first and the last designate merely *what* mechanisms are involved in the delayed effect, the second and third suggest *why* certain mechanisms show persistent action.

Taking for granted the necessity of the first or intra-cerebral theory, Köhler (23) and Rashevsky (35) have formulated constructions to meet the requirements of some of the data on set. Rashevsky's construction is developed on the basis of two physiological processes (excitation and inhibition) and a fixed topography of brain pathways. Köhler's proposal treats brain processes as fields of potential and resulting currents in a volume conductor. Both these formulations made contact with experimental observations on set only on the psychological level, rather than on the psychophysiological. For this reason such a construction cannot be unique. Given free choice, many constructions could be offered, one suspects, which would fit the psychological data. The only limitation is the requirement that the postulated process seem reasonable in the light of physiological knowledge, and this restriction is not rigorous in the present state of neurophysiology. Both formulations postulate underlying processes whose time constants are uncertain. It is necessary in them, therefore, to employ *ad hoc* time constants, so that there is no confirmation of theory in the temporal coincidence of the empirical curves with their rational ones. The contribution of these analyses may be described, therefore, as the elaboration of an hypothesis rather than the confirmation of one.

Some recent work on the neurophysiology of the higher centers also seems to lend plausibility to the intra-cerebral hypothesis. Dusser de Barenne and McCulloch (8, 9), cited by

Mowrer in support of the theory, have observed prolonged discharges from the cortex even after it was isolated from lower regions. Lorente de Nó (25) presented some evidence of the existence of self-maintaining neural excitations. Such processes have not been examined during the condition of set; and it remains to be shown, therefore, whether they operate during set, and whether they are the only mechanisms which do operate during set.

The series of experiments by Mowrer, Rayman and Bliss (29) and Mowrer (30) was thought by the writers to force the adoption of the intra-cerebral hypothesis by demonstrating the non-effectiveness of factors located anywhere else. The method was to measure reaction time to a less expected stimulus when the subject had the habit of being set for another stimulus calling for the same response. The presumption is made that the motor adjustment for the expected stimulus would be the same as that necessary for optimum reaction to the unexpected stimulus since the response was the same. Since the results were that reaction time lengthened when the stimulus was shifted, the writers concluded that the lengthening could not be peripherally determined, and therefore, must be central.

To meet the objection (13) that sense-organ adjustments probably differed in the two stimulus situations, Mowrer tried the same experiment with stimuli of the same modality and found no lengthening of reaction time. Believing that such a result was inconclusive, he experimented with a shift from an auditory to a vibratory stimulus, since he thought no sense-organ adjustment could operate to affect vibratory stimulus reception. The recorded lengthening of reaction time is believed by Mowrer to confirm his original position. Evidently, however, he overlooked the known effect of muscular tension on vibratory sensitivity (4, 10). This series of experiments actually shows a lengthening of reaction time to occur when there is a shift from one sense organ to another and not when the shift is within a sense modality.

Sense-organ adjustment as a factor in set cannot be dismissed, nor it seems can other motor adjustments be considered as finally disposed of. The presumption that pressing a key in

response to one stimulus is exactly the same reaction (on the motor side) as pressing the key in response to another may possibly be contrary to fact. Another assumption underlying this discussion is that all phenomena of set have the same locale, so that if one characteristic is absent from one locale, it may be supposed that all others are likewise. Because of such considerations, it appears that a conclusion in favor of an exclusive central locus of set would be premature. [Of course, there is no doubt that central mechanisms are functioning in some manner during the condition of set; but Mowrer's own conclusion is, ultimately, that motor adjustments are sometimes absent.]

With regard to sensory mechanisms, the work of Mowrer and his collaborators offers the suggestion that they play a role of some importance (although Mowrer prefers the interpretation that his effects arise in the sensory spheres of the cortex). The matter has been investigated further in the recent study of McClelland (26) with reference to the time error. He found that the time error for visual magnitudes follows the expectation from the after-effects of visual stimulation. Similarly, he suggests, persisting muscular tension in weight comparisons would affect proprioceptor sensitivity to the stimulation provided by the second weight lift. Thus, the set would actually be determined by conditions at the point of entry of the stimulus. If this explanation were extended to cover the reaction time situation, one would need to look more to the reflex action of accessory mechanisms, since the stimulus characteristics of the warning signal do not affect the condition of set so far as known. Such an explanation is offered by Woodworth (38) for the shorter reaction time to cessation than to onset of a stimulus.

No doubt, because of the greater accessibility of the phenomena, there are more comparisons of set with muscular action than with either central or receptor processes. These have usually been associated with the second and fourth conceptions of the psychophysiology of set. Earlier studies (reviewed elsewhere (5)) gave some indications of the relationship; and since the introduction of action potential recording the subject has been further advanced, both with respect to the reaction time ex-

periment and the comparison of stimulus magnitudes. The time sequence of the muscular action can be determined, sometimes even simultaneously with a determination of the set, by psychological means.

With regard to the simple reaction time experiment the following relationships have been demonstrated (5):

1. Muscular tension (in the chief reacting muscle) is greater during set, and varies with the degree of set. The muscular action thus exhibits the definitional characteristic of set.

2. Set and muscular tension vary in a closely similar manner with respect to time when the Breitwieser-Woodrow method of plotting is used. That is, tension at the end of the fore-period varies with the duration of the fore-period in the same way that set does. Maxima occur at the same time.

3. There is a similar variation in set and in tension as both are affected by practice.

4. An additional comparison may be made between tension throughout the fore-period and Mowrer's curve on the development of set during the fore-period when its termination is not expected. In five of the six subjects studied (5) tension was found to begin its increase after the lapse of something like the ordinary reaction time and continue to increase until, probably, the expected termination of the fore-period. Mowrer's curve of set, as previously described, behaves quite similarly.

With regard to the judgment of stimulus magnitudes (in the case of lifted weights) the following relationships with muscular tension have been shown:

1. The ratio of muscular activity during the lift of the comparison weight to that during the lift of the standard weight varies with comparison weight magnitude in the same manner as does the proportion of judgments of greater (33).

2. This ratio and the proportion of judgments of greater depend on two factors: the magnitude of the comparison weight and the level of muscular tension prior to the second lift. (With this latter factor the variation is inverse if one considers the proportion of heavier judgments.) Thus, the level of prior muscular tension plays the same role as that attributed to set as it affects the lifting of the second weight.

3. The level of tension after the first lift and prior to the second varies in time in the same manner as the set (Freeman and Sharp, 15). There is a "super-normal" phase corresponding to the period of positive time error, and a phase of depression coinciding with the period of negative time error. (The tension curve also reveals a brief episode in which a "normal" level of tension is reached prior to the positive phase

(15, 33). Were it possible to introduce the second stimulus at this point, a judgment series with practically no time error might be expected to result.)

It is not to be thought, however, that the level of muscular tension must depend exclusively upon the lapse of time since the lifting of the first weight. In fact, it appears that the level of tension prior to the lifting of the first weight tends to maintain itself through the critical period. Factors prior to and extraneous from the immediate weight-lifting may, therefore, be expected to influence this condition of muscular tension.

Unfortunately, muscular tension during stimulus judgment in other modalities has not been studied. It is very likely that increments in tension would be found; but where they may be, or whether they show the same relations as those found in weight judgment, can hardly be predicted.

So far as experimental work has gone there appears to be a good correspondence between the requirements of the set concept and the state of tension, both as to definitional and secondary characteristics. When a relationship between two variables appears, a normal scientific confirmatory procedure is to reverse the experiment, making dependent and independent variables exchange roles. The technique in this case would be to determine whether set is influenced by intentional variations in muscular tension. Historically some of these experiments were actually performed first. It is established by a number of experiments (1, 11, 14, 22) that increasing the level of tension shortens reaction time, or, we could say, improves set. Freeman presents evidence that such an increase has maximal effect at particular time intervals. The facilitating effect does not, however, increase indefinitely with further increments in tension; but as seems to be the general rule in such relationships, it reaches a maximum. In the weight-comparison experiment the introduction of a lift interpolated between the standard and comparison might seem to offer a comparable experiment. It is, however, uncertain what the effects of such interpolation might be on muscular tension. It must be recognized, of course, that devices used in the reaction time experiment for increasing muscular tension

have other effects as well (such as increasing proprioception), a fact of some significance in the final interpretation.

If within the limits of the experimental situations there is a co-variance of set and muscular tension in certain localities, is it then reasonable to identify this tension with set? Logically, of course, other interpretations are available and the facts will hardly support such a simple conclusion. In the first place, as observed elsewhere, the correspondences discussed are between averages, and the associated dispersions are probably too large to attribute them simply to experimental error. In the second place, the experiments of Mowrer and his collaborators show that it is possible under certain circumstances to divorce set from local tension of the reacting muscle. It is more appropriate, therefore, to conclude that there is a partial identity between the two. A measurement of local muscle tension, in other words, represents one part of the effective physiological process of set. In that tension, it may be said, we have a pre-excitation or post-excitation of a muscle very much like that found during the voluntary contraction itself. (Latency, duration, and form of action potential are similar.) This fact suggests that the physiological process of set as a whole shares the characteristics of a simple voluntary response. Such a response is known not to be confined to a single motor unit, or even to a muscle group, but to consist of a pattern covering most of the organism (7). We may presume, subject to future experimental check, that such is the case with the physiological process of set, and with the measurement of more of the pattern we might expect to find a more complete correspondence with the psychological characteristics of set. This last proposition, of course, implies that the local effect is not the sole feature of importance and that the remote parts of the pattern of excitation are of some consequence in influencing the action. It is possible, at least, that such may be the case. For example, a total set pattern with its focus in the speech muscles might involve a contraction of the finger extensors also; yet this pattern would probably not be so favorable to a finger reaction as one whose focus was in the finger extensors and periphery in the speech muscles. (Considerations of this sort are

quite reminiscent of certain problems of physiology of brain where it is believed that excitation patterns rather than localities are determinants.)

Pursuing this line of thought, we should consider the actual state of the individual who is participating in a reaction-time or stimulus-judging experiment. He is certainly not occupied solely and continually with the stimuli which the experimenter is providing, but is very much under the influence of stimuli which the experimenter would regard as distracting. He is subject to a continual flux of patterns of response, only some of which are optimal from the experimenter's point of view. Yet most of these would produce some increment of tension in any locality sampled. Facilitation might occur in proportion as the set coincides in pattern with the expected response. Thus, the correspondence between a set and local tension would be imperfect. Herein also lies an explanation of the results of Mowrer, *et al.*, which needs some investigation. In the action pattern for finger movement in response to vibration, for example, the results obtained would be what we should expect.

The existence of muscular excitations certainly implies the presence of neural processes, since these would be the only agency which would excite the muscles in the time required. Neural processes must also play a role in maintaining the muscular tension, since a muscle once excited, as in a reflex, does not itself maintain excitation over the period as observed in the set condition.

A number of questions regarding the relation of the neural processes to muscular processes need to be considered. Unfortunately, there have so far been no observations on any neural process aroused during the condition of set, and one can only resort to inferences. It can be presumed with some certainty, however, that there are neural correlates to the muscular patterns which at certain levels at least would themselves be similar patterns. The system of efferent nerves would contain at a given moment the pattern to appear next in the musculature, the spinal efferent tracts the next, etc., thus giving a "dimension" of depth to the pattern.

The co-relative question, whether the musculature mirrors all relevant neural events, is not so easy to answer except in a doctrinative manner. An answer could be given only if it were determined whether or not all characteristics of set are discoverable in muscular action, or whether during set there exist neural processes which could not affect the periphery. Since the appropriate neural processes have not even been identified and work on muscular processes has barely begun, hope of a final answer is quite distant. At present, all that can be said is that relevant muscular processes have been found for a number of the requirements of the set concept, and have not as yet been ruled out in any test case.

The second of the four proposals for a psychophysiology of set is a theory of the relationship of neural and muscular processes. There is in the case of set, as in all psychological processes of greater than minimum duration, the problem of accounting for such a duration. Answers of two sorts are usually given: after-discharge of an excited locale, or the activation of a reverberatory circuit somewhere in the organism. As pointed out, after-discharge of sense organs in the case of set is a possibility in some cases. After-discharge of a neural center has likewise been suggested. Reverberation, of course, might take place either within the nervous system or between central and peripheral regions. In view of the known peripheral phenomena, Freeman (12) has formulated a theory of peripheral reverberation and applied it to a wide range of phenomena. In a condition of set there are muscular contractions which doubtless excite proprioceptors and it is not unlikely that impulses therefrom contribute to the maintenance of the neural excitation which is responsible for the muscular contraction in the first place. In this theory, the muscular tensions existing during set are characterized as tonus, or postural substrate, and the final response as phasic. It is doubtful, however, whether a clear distinction can be made between those two physiological processes (6), and it would seem just as well to regard them as fundamentally the same process. How the reverberation dies out, or undergoes

changes in strength, is more of a problem, though perhaps not an insoluble one.

There is a third possibility for maintenance which, though rather unorthodox, deserves some exploration. In some situations in the reaction time experiment, for example, a stimulus itself continues through the fore-period. When this happens, the continuance of the set may be referred to the continuance of the stimulus. In fact, in such a situation where the reaction is made to the cessation of the stimulus, the set is better than under ordinary circumstances. It is conceivable that in the general case there is present during the fore-period a continuing stimulation similar to that in the off-reaction. The warning signal might initiate activity in the whole response system. The system being already somewhat activated, it might be more sensitive to appropriate impinging stimuli (such as the fixation point, background noise, or whatever might be present or even notably absent). Adjustment of receptor accessories would assist in maintaining stimulus reception. Once excited, then, the reaction system would be maintained in excitation by the surrounding stimuli which are always available. A similar account could be given for the set in the stimulus-judgment situation. As in the case of other theories of maintenance, there is here no explanation of the changes of set with time, but it is doubtful whether speculation on the point is now profitable.

In the case of stimulus-magnitude comparison there are still further complexities, since there are two responses rather than one following the initiation of the set. Nor is the final response (the subject's report) directly consequent upon the set. The evidence referred to shows a good correspondence of judgments with the ratio of the two excitations; but this likewise cannot be regarded as the immediate determinant of the judgment, since one of the excitations, at least, is no longer existent in its full form at the time the judgment is determined. We may say that the set is a direct determinant of the response to the second stimulus; the judgment is evidently a function of that response. A simple theory of events from that point would be that the excitation increment produced by the second stimulus is the di-

rect "stimulus", as it were, to the judgment, a large step-up being conditioned to a report of greater, and a small one to a judgment of less. Somewhat less simple would be the view that the size of the increment in relation to the absolute level would be the direct determinant. The more usual view—that the judgment is determined by the relative intensities of two trace excitations in the after period leads to a number of difficulties, such as discussed in Peak's analysis (34).

On the basis of the evidence considered, it appears likely that the condition of set is an active process in the organism, involving sensory, neural, and muscular levels. An essential feature of the set is, of course, its facilitative or inhibitory effects. If the set is correctly understood to be a continuing excitation of a whole reaction system, the question of where and how facilitation operates probably has a very broad answer. The facilitation would occur wherever the set is—at any and all levels of the reaction system. Any tissue with a sub-threshold excitation would reach a threshold level sooner than it otherwise would, and a prior favorable pattern of excitation would assist or impede any level. To seek the "essence" of set either in the periphery or in a central locus would, therefore, be a misdirected endeavor. Similarly, the characterization of the peripheral components of set as "mere overflow" seems to be verbal legerdemain for unduly limiting the field of investigation. The peripheral phenomena are, to be sure, overflow, or better, outflow from central processes, which in turn are "mere" effects of stimulation. In the peripheral phenomena, we evidently have a set of events, fortunately observable, which are subject to certain regularities; and they possess, as has been demonstrated, significant psychological relationships.

The present discussion has been limited arbitrarily to set as defined by two experimental situations. The choice was made in part because of their similarity and in part because of the availability of pertinent measurements. Even with the topic thus restricted, one feels a good deal of hesitancy about a general formulation because of the lack of information on so many points. Yet in considering such a formulation, there always arise

questions about its application to other somewhat similar psychological phenomena. Would similar physiological processes be found in the facilitation and inhibition of reflexes, in conditioned responses, in trace responses, in delayed reactions, in threshold variations, in word associations, etc.? All of these no doubt have features in common with one another and with the reactions here discussed. There is much to be gained, no doubt, from an analytic discussion of their similarities, and still more from direct experimental comparison such as undertaken by Peak (34) for reflex inhibition and constant error. The problem of generalizing the psychophysiological account of set may well be postponed pending further research.

BIBLIOGRAPHY

1. ANGELL, F. Duration, energy, and extent of reaction movement. *Amer. J. Psychol.*, 1919, 30, 224-236
2. BREITWIESER, J. W. Attention and movement in reaction time. *Arch. Psychol.*, 1911, 2, 1-49
3. BOWDITCH, H. P., and WARREN, J. W. The knee jerk and its physiological modifications. *J. Physiol.*, 1890, 11, 25-64.
4. COHEN, L. H., and LINDLEY, S. B. The relationship of muscle tonus changes to vibratory sensibility. *Psychol. Mon.*, 1936, 47, 83-93.
5. DAVIS, R. C. Set and muscular tension. *Ind. Univ. Sci. Stud.*, 1940, No. 10, p. 30
6. DAVIS, R. C. Methods of measuring muscular tension. *Psychol. Bull.*, 1942, 39, 329-346
7. DAVIS, R. C. The pattern of muscular action in simple voluntary movement. *J. Exp. Psychol.*, 1942, 31, 347-366.
8. DUSSEY DE BARENNE, J. G., and McCULLOCH, W. S. Suppression of motor response upon stimulation of area 4-5 of the cerebral cortex. *Am. J. Physiol.*, 1939, 126, 482
9. DUSSEY DE BARENNE, J. G., and McCULLOCH, W. S. Factors for facilitation and extinction in the central nervous system. *J. Neurophysiol.*, 1939, 2, 319-355.
10. ECHLIN, F., and FESSARD, A. Synchronized impulse discharges from receptors in the deep tissues in response to a vibrating stimulus. *J. Physiol.*, 1938, 93, 312-334
11. FREEMAN, G. L. Facilitative and inhibitory effects of muscular tension on performances. *Amer. J. Psychol.*, 1933, 45, 17-52.
12. FREEMAN, G. L. The problem of set. *Amer. J. Psychol.*, 1939, 52, 16-30.
13. FREEMAN, G. L. Discussion: "Central" vs. peripheral locus of set; a critique of the Mowrer, Rayman and Bliss "demonstration". *J. Exp. Psychol.*, 1940, 26, 622-628.
14. FREEMAN, G. L., and KENDALL, W. E. The effect upon reaction time of muscular tension induced at various preparatory intervals. *J. Exp. Psychol.*, 1940, 27, 136-148

15. FREEMAN, G. L., and SHARP, L. H. Muscular action potentials and the time-error function in lifted weight judgments. *J. Exp. Psychol.*, 1941, 29, 23-36.
16. GIBSON, J. J. A critical review of the concept of set in contemporary experimental psychology. *Psychol. Bull.*, 1941, 38, 781-817.
17. HILGARD, E. F., CAMPBELL, R. K., and SEARS, W. N. Conditioned discrimination: the effects of knowledge of stimulus relationship. *Amer. J. Psychol.*, 1938, 51, 498-506.
18. HILGARD, E. R., and HUMPHREYS, L. G. The effect of supporting and antagonistic voluntary instructions on conditioned discrimination. *J. Exp. Psychol.*, 1938, 22, 291-304.
19. JACOBSON, E. Tonus in striated muscle. *Amer. J. Psychol.*, 1943, 56, 433-437.
20. JENKINS, T. N. Facilitation and inhibition. *Arch. Psychol.*, 1926, 14, 1-56.
21. JOHANSON, A. M. The influence of incentive and punishment on reaction time. *Arch. Psychol.*, 1922, No. 54, 1-53.
22. KNOTT, J. R. Some effects of mental set upon the electrophysiological processes of the human cortex. *J. Exp. Psychol.*, 1939, 24, 384-405.
23. KOHLER, W. Zur Theorie des Sukzessivvergleich und Zeitfehler. *Psychol. Forsch.*, 1923, 4, 115-175.
24. LACEY, J. I., LACEY, B. C., and DALLENBACH, K. M. Areal and temporal variation in pain sensitivity. *Amer. J. Psychol.*, 1941, 54, 413-417.
25. LORENTE DE NO, R. Transmission of impulses through cranial motor nuclei, Symposium on the Synapse. *J. Neurophysiol.*, 1939, 2, 402-464.
26. MCCLELLAND, D. C. Factors influencing the time error in judgments of visual extent. *J. Exp. Psychol.*, 1943, 33, 81-95.
27. MARCHETTI, P. V. Time errors in judgments of visual extents. *J. Exp. Psychol.*, 1942, 30, 257-261.
28. MOWRER, O. H. Preparatory set (expectancy). Some methods of measurement. *Psychol. Monogr.*, 1940, 52, p. 43.
29. MOWRER, O. H., RAYMAN, N. N. and BLISS, E. L. Preparatory set (expectancy) — an experimental demonstration of its "central" locus. *J. Exp. Psychol.*, 1940, 26, 357-372.
30. MOWRER, O. H. Preparatory set (expectancy) — further evidence of its "central" locus. *J. Exp. Psychol.*, 1941, 28, 116-133.
31. NEEDHAM, J. G. The time-error as a function of continued experimentation. *Amer. J. Psychol.*, 1934, 46, 558-567.
32. NEEDHAM, J. G. Rate of presentation in the method of single stimuli. *Amer. J. Psychol.*, 1935, 47, 275-284.
33. PAYNE, B., and DAVIS, R. C. The role of muscular tension in the comparison of lifted weights. *J. Exp. Psychol.*, 1940, 27, 227-242.
34. PEAK, H. Time order in successive judgments and in reflexes, I. Inhibition of the judgment and of the reflex. *J. Exp. Psychol.*, 1939, 25, 535-565; II. As a function of the first stimulus of a pair. *J. Exp. Psychol.*, 1940, 26, 103-115; III. Time error theories. *Psychol. Rev.*, 1940, 47, 1-20.
35. RASHEVSKY, N. Advances and application of Mathematical Biology. 1940, Chicago: University Chicago Press, pp. xiii + 214.
36. TODD, J. W. Reaction to multiple Stimuli. *Arch. Psychol.*, 3, 1-65.
37. WOODROW, H. The measurement of attention. *Psychol. Monogr.*, 1914, 17, 1-158.
38. WOODWORTH, R. S. Experimental Psychology, New York: Holt, 1938, pp. xi + 889.

THE PHYSIOLOGICAL CONQUEST OF PERSONALITY STRUCTURE

G. L. FREEMAN
Northwestern University

Introduction and Hypothesis: A friend of mine, a philosopher, once remarked, "You behaviorists have certainly gone a long way in the physiological analysis of the simpler mental functions, but your methods can never touch the complex problems of personality structure." Had he been more conversant with the field, he would have realized that the first telling blows had already been struck and that, slowly but surely, the conquest was advancing on all sides. It is beyond the scope of this review to cover the diverse lines of approach, including experimental neurosis, brain extirpation work and the study of "emotional" strains of rats. We shall confine ourselves to that limited area to which our own laboratory has given its time for the last five years—the study of individual differences in the physiological responses of human subjects to conditions of relaxation and tension. We shall also review the cognate work of other laboratories and relate the entirety with the fading approach of personality testing by questionnaire and the rising star of "projective" technique. Our purpose in this is to show the connection which physiological records of neuro-muscular activity bear to the larger problem of personality assay, and to indicate how a fundamental organizing principle can unite the physiological and psychological levels of descriptions.

Any discussion of personality properly begins with some definition of the term. We shall content ourselves by pointing out that neither its common-sense definition as "a person's social stimulus value" nor the traditional statement of "the sum total of a number of isolable 'traits'" is held in good repute. The first definition hardly distinguishes a person from a cocktail, and

the second has been outmoded by the realization that it is the particular integration or pattern of trait-responses and not the summation of measured amounts which determines the uniqueness of the individual.

The last decade has seen a gradual waning of interest in questionnaire trait-testing and a gradual waxing of interest in the more qualitative pattern approach represented by the so-called "projective" techniques. The reasons behind this shift are fairly obvious. Personality tests which question others or the individual himself have now been improved to a fine point, yet no amount of elaborate statistical treatment can increase their diagnostic value. The plain truth of the matter is that such trait scores have practically no validity when the criterion is not an artificially constructed one, but agreement with clinical data. Questions dealing with emotionality, aggressiveness and other "traits" suggest social values; hence there is considerable confusion, as with Cooley's looking glass, between the self one should see and the self as he would like to be seen. But even if these "trait" scores did represent true factors in personality structure, the total personality could not be adequately dealt with by this approach. Representation of the individual as a scattered assemblage of abilities on the punch card of a computing machine impresses only those who care more for classificatory order than for logic. Such an approach misses the point that personality emerges out of the interaction of factors and not from their summation. When only the bricks which come out of a house are counted, a person interested in reconstructing the whole can form with them either an Arabian mosque or a roadside stand.

The contrasting "pattern" approach has had its greatest advance in the hands of clinicians who practice the new "projective" methods. In the most highly developed of these methods (23), the individual responds to inkblots and so "projects" his personality pattern on a relatively unstructured medium of expression. Responses are classified according as they reveal such basic factors as "form perception," "organizing energy," and "affective drive," and the interaction of these factors determines the total "psychogram" pattern.

Such tests show promising validity in terms of other clinical data, but fall short of satisfying the scientific criteria of precision and objectivity. They give attention to the interaction of ill-defined "personality factors" at the expense of a careful analysis of the basic parameters themselves. This is an unfortunate omission.

It appears that the projective approach to personality structure is fundamentally sound, and requires only the psycho-physiological assay of reactions to displacing stimulation to show its affinity with the basic organizing principle of organismic biology—*homeostasis*.

This term covers the tendency of all organic systems to react to a disturbance in such a way as to maintain their equilibrium or constant states. When the blood-sugar level shifts beyond set limits, automatic elevating or depressing mechanisms act to preserve its essential equilibrium; when a noxious stimulus threatens its integrity, the organism acts to remove the irritant and so restore bodily balance; in like manner the "total personality" tends to see in the inkblots what they equilibrate it to see

At first glance, it may seem impossible to pass from the refined physiological analysis of the body's internal milieu to the gross psychological descriptions of the total "projected" personality. Yet careful study of the intervening link—psycho-physiological assay of neuro-muscular homeostasis—indicates not only the practicality of the step, but also its necessity in terms of the fundamental unity of the sciences of life.

Our particular approach to the problem of personality assay has involved the study of physiological reaction dynamics¹ in

¹ We shall not be here concerned with the contrasting physiological approach which seeks correlation between personality "traits" and such *static* physiological measures as body build cephalic index and basal metabolic rate. The negative character of much of this work has needlessly impeded the advancement of physiological study of personality structure. (D. G. Patterson, *Physique & Intellect*, New York: Century, 1931; G. L. Freeman, *Introduction to Physiological Psychology*, New York: Ronald, 1934.) If the recent breakdown of morphology into three basic components (endo, meso, and ectomorphy) by Sheldon (in *Personality and Behavior Disorders*, Ronald Press, 1914, p. 537) appears to hold promise of correlating with temperament traits it may be due to the influence of a common (*i.e.*, glandular) factor.

controlled laboratory situations. Overt behavior is considered as a means of discharging bodily energies developed by internal needs and associated external conditions in such a way as to maintain the homeostatic balance of the total organism. It has been our hypothesis that personality pattern has its genesis in the way in which individuals of specific biological constitution meet displacements in their potential field of stimulation and that the major parameters whose interaction determines the total pattern are discoverable through study of homeostatic adjustments to common disequilibrating stimuli. When an organism in a relative condition of rest is faced with a disturbing stimulus (unexpected pistol shot or experimentally induced frustration) it reacts in a manner calculated to restore the previous condition of equilibrium. From the standpoint of physiological reaction dynamics there are two essential phases (1) energy mobilization and (2) discharge. Individuals differ in the amount of energy mobilization and in the rate and manner in which they restore internal equilibrium through neuro-muscular action. These processes can be measured and the major factors accounting for individual differences can be ascertained. Thereafter the interaction of these factors can be related to clinically determined personality patterns and a check made upon the validity of the physiological approach.

We come thus to an examination of such evidence as is presently at hand. This may be classified as follows: (1) non-factorial studies of neuro-muscular homeostasis, (2) factorial studies of neuro-muscular homeostasis, (3) bodily basis of drive-arousal factors, (4) bodily basis of discharge-control factors, (5) bodily factors in homeostatic variability and (6) factor inter-relations in personality structure. A discussion of each of these fields will be concluded by a brief forecast covering the presumed relation which neuro-muscular homeostasis as observed in the psycho-physiological laboratory bears to total personality patterns, as observed in the psychological clinic.

Non-Factorial Studies in Neuro Muscular Homeostasis: In one of the first attempts (3) to study individual differences in neuro-muscular homeostasis, normal subjects were given a series

of electric shocks designed to reveal such quantifiable aspects of reaction as anticipation, excitability, recovery, adaptation, conditioning, and inhibition. Response curves were obtained for blood pressure, respiration, galvanic skin resistance, and movements of the stimulated area. In addition, questionnaires designed to reveal variations in "neuroticism" and "emotional instability" were administered. No high correlations were found between physiological and psychological tests, though subjects whose rate of physiological recovery from shock stimulation was most rapid tended to be rated as emotionally stable.

In a later research (7) normal adult males were subjected to several varieties of displacement, including voluntary inhibition of micturition and protracted electric shock; measures of physiological displacement and recovery were obtained from records of palmar skin resistance, muscle action potentials, and blood pressure changes. These physiological *R. Q.*'s² were intercorrelated with each other and for different types of displacing situations. Considerable correspondence was noted between these *R. Q.*'s and the individual's ranking in other tests of nervous stability (such as length of time micturition was voluntarily inhibited) and responses made on neurotic inventories. To some extent the relative ranking of *R. Q.* scores on different displacement tests appeared to be influenced by ability to re-equilibrate to one type of displacement better than to others. Furthermore, subjects undoubtedly exerted different degrees of reinforcement in reaching the same measure of overt behavior;—for example, the time criteria in the micturition test. This has been one of the great difficulties in using overt behavior criteria (as suggested by the animal work in experimental neuroses) as an index of nervous breakdown in man.

Subjects in the next *R. Q.* study (8), were trained to make right or left index finger reactions in signalling discrimination of "lighter" or "darker" to a pair of near-threshold stimuli. Shocks

² The *R. Q.* or recovery quotient is a measure derived from records of physiological activity taken before, during and after disruptive stimulation. It has been defined empirically as the ratio between the amount of increment and decrement in a homeostatic sequence occurring over set and arbitrary time limits.

were given for wrong responses and for failure to respond within a stipulated time. As with Pavlov's neurotic dog, the experimental series began with the easy discrimination of widely separated stimulus-pairs, and the difference was narrowed down until both members were alike. To test for the effects of frustration, as soon as the subject make three consecutive failure-reactions, without his knowledge he was again given the original easy discrimination-pairs. The differential between the first and second series of easy discriminations became an overt behavior index of homeostatic breakdown. This correlated negatively with skin resistance *R. Q.* scores for the group as a whole; but it is doubtful if one could make individual prediction from the covert to the overt behavioral index or *vice versa*. Another overt measure-number of trials successfully passed before the failure reaction, has been related somewhat with the *R. Q.* measures of covert physiological activity. A similar procedure has been tried more successfully with children at a complicated level of performance difficulty (16). Such data suggest that individuals who cannot long withstand experimental frustrations or whose overt behavior is most disturbed thereby recover physiological equilibrium slowly. The stable system, on the other hand, is more resilient, reacts adequately but not excessively to frustration, maintains available energy for additional loads imposed, and shows little residual tension following attempted overt reaction or removal of the disruptive stimulus. Not known, of course, are the number of axes of differentiation involved in these standardized frustration tests, or their precise relation to the total personality structure. The pertinence of physiological recording to personality study is, however, revealed by such work.

Factor Analyses of Neuro-muscular Homeostasis. Every psycho-physiologist will recognize that the ultimate test of the axes of personality differentiation is an involved statistical one. The organism must be exposed to a variety of test situations and the inter-correlations of several physiological measures must be obtained. Next must come the factoring of this complex and the positing of the least number of factors needed to account for the major variance. It is significant that until very recently little or

no attempt was made to study the physiological reactions of the person in a number of situations and to inter-correlate the measures. Lately, there has been some attempt to obtain a large number of physiological measures on the same population; but the number of subjects has been small and most of the tests static, rather than dynamic in character. The first factorial study (26) was carried out with only six subjects, and included a battery of paper and pencil personality tests as well as such static measures of physiological reactivity as basal metabolic rate, skin resistance level, and resting blood pressure. This study yielded some indication of three factors of personality differentiation. (1) A "metabolic" factor, (2) a "muscular tension" factor and (3) an "emotional liability" factor. Few paper-and-pencil tests correlated with the physiological tests, and the presence of a cretin in the group of six subjects may have accounted for the high loadings of the first (metabolic) factor. Subsequently, the same investigator (25) determined the inter-relationships between such variables as reclining and standing changes in pulse and palmar skin resistance, latency and persistence time of red dermographia, salivary output and per cent of solids, variability in respiration and heart rate, basal metabolic rate, motor reaction time, residual muscular tension after instructions to relax and ratings for restlessness, using a much larger sample of children. The results indicated two significant common factors which were called autonomic nervous system or "autonomic balance" and "muscular tension" or relaxability of skeletal musculature. The variables correlating with each factor were then used in deriving—by means of the technique of multiple correlation—a regression equation for estimating these factors for individual subjects.

Such work is of tremendous significance, but cannot be directly related to factors operating in more complex behavioral adjustments. It will be noted that all measures were taken under conditions of rest or minor physiological disturbance. There were no measures made in frustration situations and thus the picture is confined to factors operative in maintenance of basal activities of rest. That these same factors may also apply to dif-

ferences in reaction to complex stimulation and stress is indicated in reports by other researchers.

Only two factorial studies have yet been made in which physiological reactions to a variety of experimentally induced displacements were recorded. In one, the subjects were thirty-eight school children, differentiated by behavior ratings into "maladjusted" and "well adjusted" groups. In the other, the subjects were twenty-four male college students.

The study of homeostatic reactions of children (20) showed twenty-seven variables (from the original total of fifty-two measures) which differentiated the maladjusted and well adjusted groups; of these "galvanic (skin resistance) recovery" following stimulation proved to be one of the most highly significant. A factorial analysis of the data was performed for the eighteen cases of the "well adjusted group" and yielded three factors. The first factor was heavily loaded with a rating of emotional stability, "percent-time alpha" of the electroencephalogram and "basal muscular tension"; it was labelled by the author "a general emotionality factor". The second factor had three significant loadings, all concerned with the increment of physiological activity during stimulation; this the author labelled a "central factor" since all loadings were reflections of physiological change. The third factor contained no significant loadings.

The study of homeostatic reactions of college students (11), recorded galvanic skin resistance changes and amount of overt motor discharge (restless movements, voice level), before, during, and after four different types of stimulus displacement (unexpected pistol shot, motor conflict, verbal association conflict, pitch discrimination under distraction). In addition there were clinical ratings of "emotional balance" and self-ratings of "neuroticism", "drive", "anxiety", "lack of outward emotional control", "visceral disturbance" and "response variability."

Thirty variables were subjected to a factorial analysis. This analysis resulted in three factors. The first was heavily loaded with skin resistance $R.Q.$'s (for several types of stimulus displacement), overt "restless" movement in the basal state, and clinical ratings of emotional stability; it was labelled by the

authors an emotional factor, related to the central nervous control of overt discharge. The second factor was heavily loaded with measures of stimulus-produced increments in physiological activity and was labelled an "arousal" factor. The third factor was heavily loaded with paper-and-pencil test scores and was labelled by the authors, "self-rater neuroticism." The fact that self-estimates of "drive", "control", etc., failed to correspond with objective behavioral indices of these factors suggests that the subjects answered the personality inventory in terms of their knowledge of acceptable behavior. This defect of self-rating would be especially pronounced with psychologically sophisticated subjects such as were used in this study. Only with the variability questions (whose intent was concealed) was there evidence of correlation between the self-stimulated trait and physiological variability in actual behavior situations. This suggests that if the experiment had been designed to study the problem more fully, a significant variability factor might have resulted.

The fact that two studies, independently designed and conducted from entirely different angles, should reveal compatible results with regard to bodily arousal and the control of its overt expression transcends simple chance. Also, it appears that the studies based upon physiological variations in more static test situations also reveals the independence of "arousal" and "discharge" factors in personality differentiation. For purposes of comparison, we list all factorial studies below, together with our analysis of their major findings. It should be noted that the third or variability factor is much less certain. Studies which did not employ physiological measures have not been included in this table. But the "projective technique" factors of "affective drive" and "organizing energy" may be expressions of the same things.

FACTORS IN NEUROMUSCULAR HOMEOSTASIS

STUDY	FACTOR 1	FACTOR 2	FACTOR 3
Wenger (1)	Emotional Liability	Muscular Tension	Metabolic
Wenger (11)	Autonomic Balance	Skeletal Tension	? ? ?
Jost	Central	Emotionality	
Freeman and Katzoff	Arousal	Discharge Control	Self-rated Neuroticism

Bodily Basis of the Arousal Factor: The bodily variability of the "arousal" factor in personality structure is not too well understood. Recently, however, positive evidence has been presented by two independent sources (4, 27), for a linkage with the autonomic nervous system. In line with an early suggestion (6, 22) that this system tends to function as a unit, with the normal resting individual somewhere between the extremes of vagotonia and sympatheticotonia, it has been found possible to estimate a person's position on a quantitative continuum of "autonomic balance." The measurement of autonomic balance is relatively stable and therefore highly reliable (29). It is presumed that persons whose resting bodily economy is habitually "balanced" on the sympathetic side are given to excess excitability and sensitiveness. Nothing is known as to the influence of environmental conditioning on this factor but there is undoubtedly something more fundamental (*i.e.*, constitutional) involved. The intimate connection of autonomic function and endocrine activity is generally recognized. A promising suggestion, therefore, is that glandular differences may produce variation in amounts of available energy with which the organism can meet disequilibrating conditions.

Pathology has shown many curious distortions in personality structure which accompany the malfunctioning of the thyroid, pituitary, and sex glands. These glands have considerable to do with making bodily energies available for homeostatic adjustment. If the quantity of hormones produced is related to the size of the glands, great differences in the amount of stimulation derived therefrom would undoubtedly exist between individuals. Enormous variability has been found in the weights of the endocrine organs, and while the weight of the brain, heart and other internal organs is proportional to the total bodily weight, the endocrines show no such constant relation. When a small thyroid gland is forced to coordinate its function with a group of larger organs, this will obviously not produce the same behavior effects as when a large thyroid is balanced against a similar organ relationship.

Certain experiments have indicated a correlation between

size of adrenal, thyroid, and pituitary glands in "emotional" and "non-emotional" strains of rats. So complex are the problems of glandular balance and interrelation, however, that at present few definitive statements can be made of their exact relation to the arousal or drive factor in total personality structure. It is yet purely speculative to suggest that the arousal factors found in studies of neuromuscular homeostasis have an endocrine basis, with some individuals constitutionally excitable and others more phlegmatic. It does appear, however, that individuals differ in the amount of energy available for work. Though various bodily tissues operate in the complex, the endocrines are primarily "releasers" of stored energy for work. The fact that high metabolism has been reported to correlate with high tested intelligence and with excitable temperament (19) lends some point to the argument that hyperactive glands contribute to the energy mobilizations which support the homeostatic adjustments of total behavior. We are entering here upon a very speculative field of endocrinology, and one guess is about as good as another as to what the full relation between glandular structure and personality will ultimately turn out to be.

Bodily Basis of the Discharge-Control Factor: Throughout our study of personality differentiation, the problem related to control of the behavioral discharge of aroused bodily energy has bulked very large. The factorial analysis of homeostatic reaction to various displacements has indicated that physiological recovery is related to factors which control energy discharge or the overt expression of bodily arousal. No cues were here provided as to constitutional bases, though it was surmised that the control factor is more subject to environmental influence and training than is the arousal factor.

The experimental literature on the indices of neuromuscular control is not large. Apparently an individual may inhibit the discharge of aroused energies or express them so fully that the response re-excites. Both conditions suggest residual muscular tension, and this measure has proved to be an indirect means of estimating discharge control.

Though William James proposed many years ago an inti-

mate relation between the explosive type of personality and hyperactivity of the skeletal musculature, the suggestion was not acted upon until recently. Work with young children (5) has shown significant correlations between high muscular tension and ratings of "lack of stability", "nervousness", and "jittery reaction" made by teachers and physicians. College students (12) also showed a positive correlation between muscular tension residuals in emotionally displacing situations and ratings of irritation and lack of nervous control in life situations. The degree to which a person lacks control of muscular tension can be estimated reliably during resting activity (28). Mention could also be made of those studies (9, 21) showing the relation of undirected involuntary muscle tension to disorganized performance.

The most fruitful attack on the neuromuscular control factor is through the measurement of skeletal discharge processes in relation to the more autonomic arousal processes. In certain preliminary attempts to study these relations, evidence from normal subjects (13) suggested that individuals who recover internal equilibrium quickly (as measured by a return of palmar skin resistance measures to a pre-stimulus base) show a high degree of externalized activity (as measured by increased movement during stimulation). Since one of the alleged factors in psychoneuroses and psychotic disorder is a discrepancy between aroused excitation and its adequate external expression, the same experiment was repeated on two representative psychotic groups (14). Comparisons showed that manics as a group have greater internal arousal and greater overt discharge than normal subjects, whereas schizophrenics were aroused no more than normals. Ideational and sensory displacements effected arousal and recovery factors differentially in the two psychotic groups, but the physiological reactions were more diagnostic of the duration of the psychosis than of its classification. Because of the variability within this group, the correlation of physiological recovery with motor discharge was less for psychotics than normals.

A fundamental defect in the above studies is that the measure of overt discharge does not take into account all types available to the subject; more especially they do not attack the prob-

lem of the *specificity* of discharge. In attaining homeostatic re-equilibration more than the mere quanta of discharge needs to be considered; attention must also be given to its appropriateness.

If appropriateness is defined as the degree to which an overt response is specifically adaptive to its initiating cause, frustration tests can be designed to measure both the amount of specifically directed response, and activity in which is non-specific to the task at hand. This was the idea behind the Luria technique (21) of studying manual reactions to conflict stimuli, but quantification of the general-specific discharge-ratio was not made. Recent attempts at quantification have been made with typists (1) and with children (16) competing in a difficult discrimination test. Records were taken of the general motor discharge of wiggling of the children, and of the extent of manual movements specific to operating the test apparatus; results indicated that subjects who discharged relatively more of their aroused energies over the manual channel (which was specifically appropriate to the experimental setting) were also the ones who tended to recover equilibrium quickly, and to be left with only a small amount of residual tension.

The studies mentioned show that the control of overt discharge is a measurable and important aspect of personality pattern. Yet the bodily mechanisms primarily responsible for the adequacy or inadequacy of equilibratory discharge is not revealed. At present we can only advance a tentative suggestion.

According to the theory underlying in this approach, overt behavior is considered as a means of discharging in a coordinated fashion bodily energies developed by internal needs and associated external conditions. Behavior is also regarded as functioning to maintain homeostatic balance of the total organism. The specificity and adequacy of discharge is largely set by training; and this presumably operates through some centers in skeletal muscle control in the central or extrofective nervous system. A guess would relate discharge-control mainly to frontal lobe function.

Many studies have reported on the lack of directional motor

control, the absence of inhibition, and the excessive skeletal discharge of persons and animals deprived of the frontal lobes. Also there is the evidence of behavioral improvement in certain psychotics where frontal lobotomy has freed the patient from too great inhibition of reaction-discharge (19). The central nervous system would seem to control the motor discharge factor just as the autonomic system tends to control the arousal factor in personality structure. The central nervous system and its associated exteroceptors appear to operate through the elevating and depressing processes of excitation and inhibition; that is, to discharge a given energy mobilization certain motor pathways must be excited and others inhibited. Either a spread of inhibition to all channels of discharge, or the complete ascendancy of excitatory processes would interfere with the orderly progress of homeostatic equilibration.

The question of significant constitutional differences in discharge-control may not be answered at the present time. It is probably true that the more the internal physiological arousal, the more difficult will be the control of its externalized expression. The tendency toward overflow and diffusion of energy in non-specific discharges will be greater in persons highly aroused by stimulation. But it is by no means axiomatic that as bodily arousal increases, control over its overt expression deteriorates. The internal and external aspects of the homeostatic process are somewhat independent factors. Some persons, due perhaps to inherently powerful central nervous control, are always able to channelize aroused energy and express it in a coordinated fashion. Though highly aroused internally, they are still judged as "poised", "masters of the situation" and "resourceful". Because of this independence of arousal and discharge factors, it would be possible to produce either a genius or a psychotic by similar conditions of arousal.

Bodily Factors in Homeostatic Variability: Another prominent factor in personality differentiation appears to be the variability of the processes used in homeostatic re-equilibration. While not of the same order as autonomic "arousal" and central "discharge" factors, its measurement may have the greatest practi-

cal significance. Some individuals, when displaced by similar stimuli mobilize and discharge their energies with considerable constancy. Both the amount of arousal and the character of the behavioral discharge can be predicted with considerable accuracy. Other individuals are highly variable on one or both of the factors. Not only is their behavior erratic and unstable; they also tend to fluctuate in the maintenance of basal levels of reactivity. The variability in types of overt reaction might be due to errors in early conditioning, as where an act approved one day might receive severe punishment the next; but the inconstancy of basal maintenance processes is not so easily laid at the door of "experience." Presumably some inadequacy of hormone control, autonomic balance or other constitutional factor is responsible.

It might be that unstable equilibrators are not well knit or well-integrated physiologically. Another suggested explanation is that they are attempting to stabilize their internal milieu in relation to the external world at an abnormally high level or low level of energy expenditure; this would place them, in terms of elevating or depressing mechanisms, well on the plus or minus side of the normal range of autonomic balance. It is presumably true that organismic stabilization is more difficult to maintain at the extremes of bodily reactivity, and that reactions will be more variable in such cases. It should be noted that whereas normal and psychotic groups are not significantly differentiated in terms of physiological arousal and recovery or resting activity level, psychotics as a group are less variable (*i.e.*, more rigid) in their reactions (14). The greatest degree of homeostatic variability appears to come not from the normal or psychotic groups, but from the psycho-neurotic classification. The latter group, more than any other, has difficulty in maintaining homeostatic constancy even under conditions of rest. While the suggestion needs confirmation with more cases, it would appear that physiological variability may be a very significant index of unstable personality structure. In this connection, it is worth noting that variability of basal physiological activity has been found to be positively related to rater judgment of amount of overtly expressed action (18).

Factor Interrelations in Personality: The beginning of this article emphasized the "pattern" concept of personality, but opened the way to analysis of the major axes of differentiation through study of neuromuscular homeostasis. Obviously, the present limits of the work are not adequate to a complete appraisal of the individual personality structure. With the idea of attacking "first things first", situations demanding energy mobilization and discharge were purposely set to emphasize the emotional rather than the intellectual aspects of personality. The extensive work in intelligence testing has already given considerable understanding of that major parameter of differentiation and its subfactors. It is presumed that, on the physiological side, an individual's discriminative abilities are somehow related to the capacity of his higher nervous centers to use the products of metabolism to advantage (24).

It is suggested that the way in which quantitative gradations in such factors as discriminative capacity, drive arousal and discharge control are interrelated determines the uniqueness of the individual personality pattern.

Assuming that these three factors present normal curves of continuous distribution, the great majority of the population will fall between the extremes of high and low values on each factor; and as a result, the "normal" personality pattern will show a fairly equitable interaction of median arousal, control, and discriminative values. We may not assume, however, that a given individual will possess all basic factors in equivalent degree. Such a case is the exception rather than the rule, and this fact accounts for the tremendous variations in personality pattern from a limited number of basic variables. In an individual where one basic factor is heavily disposed on either the positive or the negative side of the median value, this will undoubtedly outweigh median values of the other factors in the total personality make-up. That one factor may take the lead over others in determining personality structure, and even have some connection with morphological development, has been indicated by recent studies * of

* See W. H. Sheldon, in *Personality and Behavior Disorders* (New York, Ronald, 1944), p. 537.

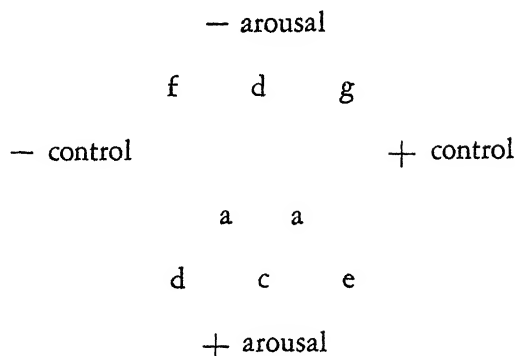
persons who are viscerally, skeletally, or cerebrally dominant.

We have not been so concerned with the extremes of inter-factor dominance as with some general scheme for visualizing the relation of the major axes of personality differentiation with each other for showing the individual's position with reference thereto. This is possible by constructing the various hypothetical axes of differentiation orthogonal to each other, and then using a multi-dimensional sphere whose radius is unity to include all members of the population within its confines. The greatest number of people would be represented as dots near the center where all axes intersect. The deviates would be represented in more outlying areas. Although an over-simplification, such a figure could indicate that the individual is always an organized totality and not an aggregate of conflicting positions on linearly represented normal curves of "trait" distribution.

Our present explorations of neuromuscular homeostasis provide, of course, an insufficient basis for the actual construction of the schematic figure just referred to. We are by no means certain how many parameters will ultimately be required to account fully for variations in personality structure. Even the presumed factor of discriminative capacity was largely ignored in existent tests. But this had the advantage of permitting us to survey the interplay of two factors, drive arousal and discharge (emotional) control, with considerable profit. The patterning of these factors has been observed in connection with selection tests of civilian and military personnel (15), with psycho-neurotic cases (14) and in other tests not formally reported upon. The results, though sketchy and incomplete, indicate that certain well-known clinical "types" can be identified with reference to two known axes of homeostatic differentiation.

In the figure below, we have represented by letters, A, B, C, etc., persons possessing different personality patterns due to the interaction of drive arousal and discharge control factors. Before discussing each particular pattern designated, it should be mentioned that we are assuming "arousal" and "discharge" measures to be a typical average for each case discussed. But it is general differences in neuromuscular homeostasis which enter the present

scheme, and not the specific differences caused by the stimuli of varied interest and significance.



A represents the position of the superficially calm and collected individual of median arousal whose higher nervous centers, naturally or by training, exercise a high degree of inhibitory control over skeletal responses. This tends to block socially inappropriate or untimely overt discharge. Aroused excitation may then be directed toward the smooth muscles and other tissues which are not under such a degree of inhibitory control as are the striped muscles. Heightened activity in these latter tissues may help relieve the higher nervous system of some of the afferent load. Such an individual may make some overt adjustments which society approves, but he is physiologically unbalanced. His nervous system is still being stimulated, due to skeletal, humoral and visceral activity, and if these tensions-effects pile up and become strong enough to push the level of organismic function beyond optimal limits, this may cause violent overt reactions (as in the axe murders); as an alternative the individual may develop a serious colitis or gastric ulcer and never realize the defective homeostatic which is the basis of his disease. All consequences come back, ultimately, to a sufficient energy mobilization with excessive inhibitory control over its expression.

B is the superficially nervous and individual whose direc-

tional control of overt skeletal discharge is fundamentally weak, or at least underdeveloped. He fidgets, engages in useless and excessive movements. While he is often the "jittery type" given to anxiety, B is likely to have a calm digestive system and lower activity levels in smooth muscle and gland tissue than does the individual described in A. Several consequences may be inferred for B, but the essential here is one of average energy mobilization with poor directive control.

C is an individual whose autonomic mechanism of arousal are fundamentally weak. He has not mobilized sufficient energy to allow his higher nervous system to function appropriately in difficult situations. He "gets mad, but not mad enough", and is often forced to use socially inadequate methods of ideomotor (phantasy) discharge for want of energy to carry out more appropriate reactions. The essentials of this pattern are possession of average directive control but low energy mobilization.

D is the "energetic" person with excessive energy mobilization and average discharge control. In contrast to the person mentioned in C, he gets *too* mad in a difficult situation, but may release the extra energy into other channels. The essential characteristics of this pattern are a high level of energy mobilization with just sufficient directive control to keep the lid on.

E is an individual of high control and low arousal. An exaggerated case of C, such a person would be prone to develop a serious personality disorder due to the great discrepancy between arousal and control factors in neuromuscular homeostasis.

F is likewise an individual with serious discrepancies between arousal and control factors. An exaggerated case of B, his high arousal and low control may result in the manic type of psychotic personality.

G is an individual of both low energy mobilization and low directive control. Such a person may show a proneness to develop neurotic disorders of the type that feature withdrawal from disturbing situations.

H is an individual whose high control and high arousal (if accompanied by sufficient discriminative capacity) may produce works of genius. He has the potentialities of fine coordination

of behavioral discharge in the face of excessive energy mobilization.

The question arises as to which of the personality patterns described above should be regarded as "abnormal". All are potential causes of concern. In theory at least, only the persons represented around the center of our graph have the most appropriate relation of directive control to energy mobilization and so can meet adequately and recover easily from displacing stimulation. From such a center, deviate patterns are considered more or less acceptable according to the culture in which they appear. In a competitive society such as ours, persons falling within the shaded area HEG are more likely to be regarded as "normal", whereas those falling within the area GFH are usually classed as abnormal.

We should be very careful in using this simplified presentation of the interlocking action of drive arousal and discharge control factors in predicting all behavior consequences. Several other axes of personality differentiation affect the total pattern. Chief among these, we presume, are the factors of response variability and discriminative capacity. Lack of intelligent management may upset every mobilization-discharge relationship that has been discussed. Instability can appear in homeostatic relations, maintained at any level of arousal and for any degree and type of discharge. Bodily arousal and its equilibration can be accomplished by relatively automatic mechanisms in some persons, almost regardless of the reactivity level that is met by the displacing stimulus. These persons should show stable neuromuscular homeostasis. Others may have to use higher order equilibratory adjustment mechanisms, the control of which is more variable and acquired.

Physiological Reaction Dynamics and Personality Assay: We come finally to the question with which we started: What do physiological methods contribute to the understanding and diagnosis of personality? It is our contention that studies in neuromuscular homeostasis supply an essential base for the psychological assay of personality structure.

We began by pointing out that most psychologists now refer

the uniqueness of the individual to the particular *patterning* of several "basic factors" or "parameters of differentiation". The fundamental weakness of this approach lies in the fact that the alleged factors are vague and ill-defined. Assays based on the analysis of verbal responses of the individual and of others who observe him are not likely to solve the problem. At this level of attack, it is almost impossible to segregate basic from superficial characteristics. That is why psychologists have long sought "bodily" correlates of their alleged personality factors, and why they should welcome more finite analysis in more limited and controlled experimental settings.

The physiological study of reaction dynamics attempts to simplify the problem of personality assay by observing individual differences in neuromuscular homeostasis under standardized conditions of displacement. It makes no prior assumption of psychological "factors" whose bodily correlates it hopes to discover,—an approach which has been very unfruitful. Rather it proceeds from the reserve direction. Psycho-physiological factors are assigned to cover the major variations in the records of overt and covert behavior, and these are presented as a basis for checking the validity of factors derived from the psychological level of description.

The move toward objective behavioral measurement as a base for personality assay is gaining headway on several fronts. We have not dealt with those excursions which confine their measurements exclusively to overt levels of response, because we have felt that covert indices of energy mobilization and discharge are more independent of the peculiarities of previous experience and the displacing situation. We are seeking relatively persistent and typical bodily trends in neuromuscular homeostasis. And these, we propose, should be used in checking the validity and "basicness" of psychologically tested "traits" such as "suggestability", "dominance" and "emotionality."

Practically nothing has yet been done with this second part of the program, but present indications are promising. For example, it has been shown (11) that various paper and pencil tests correlate with each other to produce a factor of "self-rated

neuroticism" but fail to show any relation with objective indices of emotional arousal and its control. On the other hand, certain clinical ratings of emotional instability are found positively related to defective neuromuscular homeostasis. Thus is provided a tool for sorting out promising psychological test procedures from the numerous and contradictory trends that flood the field today.

A person looking for signs of constructive order should not neglect the current progress of projective techniques. We feel that this general approach to personality structure holds considerable promise and has a direct affinity with the work on physiological reaction dynamics, due to its implied recognition of the principle that all behavior is homeostatic-regulatory. Personality patterns built up during the life history of the individual are the resultants of the interaction of aroused excitation patterns and varying degrees of thwart or frustration imposed upon their overt expressions by environmental circumstances. To assay an existing pattern, one method is to present the subject with a relatively unstructured medium of verbal expression (the essence of projective technique); another method is to present the subject with simple sensory disturbances for the purpose of observing persistent trends in physiological reaction. In both situations, the individual is revealing his "personality pattern" by his homeostatic adjustments.

More intimate details of the alleged relation between physiological and psychological levels of personality assay must await a combination of the two approaches on the same group of subjects. Therein will be the true test of the efficacy of the physiological attack.

It is not thought that records of blood pressure and electrodermal responses will ever entirely supplant the grosser and less refined methods of personality assay. Those who see a galvanometer as the flying carpet of psychology should look for the glue under the rug. Physiological measurement is likely to prove too cumbersome and difficult of simple interpretation for widespread use in clinical situations. But even if these methods must remain in the laboratory, it seems they will still be the essential means of validating grosser methods of assay. Such crude clinical

methods as show correlation with the basic variables of neuromuscular homeostasis could then be used in practical diagnostic procedures, and those psychological "trait" tests of low validity could then be discarded, along with physiognomy, phrenology, numerology and other pseudo-scientific methods of personality assay.

Our knowledge of personality stands to gain from any procedures which link the field with the dynamical constructs of organismic biology. To this high purpose, we recommend the further exploration of neuromuscular homeostasis with the aid of physiological recording techniques.

BIBLIOGRAPHY

1. ARNOLD, M. A. A study of tension in relation to breakdown. *J. Gen. Psychol.*, 1942, 26, 315-341.
2. CANNON, W. B. *The Wisdom of the Body*. New York: Norton, 1940
3. DARROW, C. W., and HEATH, L. I. Reaction tendencies relating to personality, in *Studies in the Dynamics of Behavior* (K. S. Lashley, Editor), University of Chicago Press, 1932
4. DARLING, R. P. Autonomic action in relation to personality traits of children. *J. Abn. and Soc. Psychol.*, 1940, 35, 246-260.
5. DUFFY, E. Tensions and emotional factors in reaction. *Genetic Psychol. Monograph*, 1930, 7, p. 65.
6. EPPINGER, H., and HESS, I. Vagotonia (translation). *Ment. and Ner. Dis. Monograph*, 1915, 20
7. FREEMAN, G. L. Toward a psychiatric plimsoll mark; physiological recovery quotients in experimentally induced frustration. *J. Psychol.*, 1939, 8, 247-252.
8. ———. A method of inducing frustration in human subjects and its influence upon palmar skin resistance. *Amer. J. Psychol.*, 1940, 53, 117-121.
9. ———. Postural accompaniments of the voluntary inhibition of micturition. *J. Exper. Psychol.*, 1938, 23, 45-61.
10. FREEMAN, G. L., and KATZOFF, E. T. Methodological evaluation of the galvanic skin response, with special reference to the formula for R. Q. (recovery quotient). *J. Exper. Psychol.*, 1942, 31, 239-248.
11. ———, and ———. Individual differences in physiological reactions to stimulation and their relation to other measures of emotionality.
12. ———, and ———. Muscular tension and irritability. *Amer. J. Psychol.*, 1932, 44, 789.
13. ———, and PATHMAN, J. H. The relation of overt muscular discharge to displacing stimuli. *J. Exper. Psychol.*, 1942, 30, 161-174.
14. ———, and ———. Physiological reactions of psychotics to displacing stimulation. *Amer. J. Psychiat.*, 1944, 100, 406-412.
15. ———, MANSON, G., KATZOFF, and PATHMAN, J. H. The stress interview. *J. Abn. and Soc. Psychol.*, 1942, 37, 427-440.
16. HAGGARD, E. A., and FREEMAN, L. G. Reactions of children to experimentally induced frustrations. *Psychol. Bull.*, 1941, 38, 581.

17. FULTON, J. F. Levels of autonomic function, in *Relation of Mind and Body*. Baltimore: Williams and Wilkins, 1939.
18. HERRINGTON, L. P. Relation of physiological and social indices of activity level, in *Studies in Personality* (Quinn McNemar and M. A. Merrill, Editors). New York: McGraw-Hill, 1942.
19. HINTON, R. T. The role of the basal metabolic rate in the intelligence of ninety grade-school students. *J. Educ. Psychol.*, 1936, 27, 546-550.
20. JOST, H. Some physiological changes during frustration. *Child Develop.*, 1941, 12, 9-15.
21. LURIA, A. R. *The Nature of Human Conflicts*. New York: Liveright, 1932.
22. PATEK, A., and WEISS, S. Tests of tonus of the autonomic nervous system in arterial hypertension. *New Eng. J. Med.*, 1931, 205, 330-334.
23. RORSCHACH, H. *Psychodiagnostik*. Berlin: Huber, 1932.
24. WASHBURN, M. F. Engines, energy and the engineer. *Amer. J. Psychol.*, 1929, 41, 322-326.
25. WENGER, M. A. A study of physiological factors; the autonomic nervous system and the skeletal musculature. *Human Biol.*, 1943, 14, 69-84.
26. ———. Some relationships between muscular processes and personality and their factorial analysis. *Child Develop.*, 1938, 9, 261-276.
27. ———. The measurement of individual differences in autonomic balance. *Psychosom. Med.*, 1941, 3, 427.
28. ———. A study of physiological factors: the autonomic nervous system and the skeletal musculature. *Human Biol.*, 1942, 14, 69-84.
29. ———. The stability of autonomic balance. *Psychosom. Med.*, 1942, 4, 94.

RECENT DEVELOPMENTS IN CONDITIONING *

J. DONALD HARRIS

Medical Research Laboratory

U. S. Submarine Base, New London, Conn.

Inasmuch as many psychologists have recently turned to problems of more urgent nature, this review will not confine itself to studies appearing within the past year, but will cover instead the five-year period beginning roughly with 1939 and extending through 1943. A few earlier references are included for continuity's sake. It will thus overlap by perhaps a year the bibliography of Hilgard and Marquis (1940), but not the several reviews published a year or two previously. Space will be devoted to the evidence recently provided concerning several significant questions rather than to a bare report of every experiment performed within the period.

Is conditioning merely a "switching" job?

More and more it is being emphasized that, if conditioning is to be explained, other considerations must supplement what is known as "stimulus-substitution", the notion that the conditioned stimulus comes merely to act in lieu of the original or unconditioned stimulus in calling out some particular response. In this country, where motor responses have been studied more widely than glandular, it was early noticed that the conditioned response may not closely resemble the unconditioned reflex—may indeed involve movements of the antagonistic muscles. Even with Pav-

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lov's salivation experiment, in which the principle of stimulus-substitution seems clearest, a careful description of behavior accompanying salivation reveals features for which this one principle alone cannot account¹ (Zener, 1937).

In seeking alternatives for a simple stimulus-substitution, one observation, from dogs being conditioned to avoid shock, seems of importance (Culler, *et al.*, 1935). It was noticed that, in the early stages of conditioning, the dog exhibited behavior which was very similar indeed to that which was actually elicited by the shock itself. Later on, when the animal was so well trained that he seldom experienced any shock, the conditioned response bore only the faintest resemblance either to the unconditioned response or to the initial development of the conditioned response itself. From this observation arose the suggestion that during the course of conditioning, the conditioned response may originate as the result of a relatively uncomplex substitute process; but that its elaboration may involve another principle related not only to the shock, but to the *avoidance* of shock as well.

Schlosberg (1937) in general concurs with this position. He had found that rats could learn better to move their paws upon application of the conditioned stimulus if they could not avoid shock than they could if such a movement resulted in no shock. His results thus far support the substitution theory. A discrepancy between his results and some experiments of Hunter and of Culler, whose animals readily learned to avoid shock, led Schlosberg to believe that avoidance vs. non-avoidance is a crucial typological difference. This is probably true. On the other hand, Munn (1939) finds little difference in rats between avoidance vs. non-avoidance—in fact the latter has some slight superiority.

The discrepancy between Schlosberg's results and other data may, it seems to the writer, be easily explained in terms of the characteristics of the response involved. Leg-withdrawal in the rat, accompanied as it must be by severe restriction of bodily ac-

¹ For a lucid and provocative statement of a theoretical position in which something akin to a substitutive process underlies not only conditioning but all types of learning, the reader is referred to Guthrie (1935, 1940, 1942, a, b). Unfortunately his argument is not readily susceptible to experimental attack.

tivity, is subject to many vagaries—positional, motivational, inhibitory—which cannot always be taken into account.

Suffice it to say that, at least in the first stages of conditioning, behavior is observed which may best be explained in terms of relatively simple stimulus-substitution; but thereafter other explanatory principles may need to be added.

Later writers have made these points with good effect. Hilgard and Marquis (1940), for example, considering that neither stimulus-substitution nor the age-old principle of effect is adequate, advance a third principle, of "expectancy", taken from some work of the first-named author. According to this principle, reinforcement occurs when an expectancy is fulfilled. But Stephens (1942) rightly calls attention to the fact that "expectancy" in this sense cannot operate in cases where an undesired expectancy is *not* confirmed, nor where an unexpected but utile outcome is reached. Stephens' statement that at first the conditioned response resembles the unconditioned response but that later on it combines adaptively with the unconditioned situation, is presented as a "two-phase" theory of reinforcement. He notes that the two principles, substitution and effect, make a valuable biological team: substitution provides a response which may or may not be adaptive (though more likely the former), while effect insures that sooner or later adaptation will be forthcoming. In his words, "By using a combination of these two principles . . . (nature) can secure the benefits of each, and by using them *in the sequence* suggested by the two-phase principle (*italics his*), she can achieve the ultimate in efficient adaptation. By using the two-phase principle, nature says to us, in effect, 'When you need a new way of responding to a CS, first of all try the UR elicited by the UCS. There are 9 chances out of 10 that this is almost the very response you need, and that a little polishing by the B phase will make it exactly what you want. If this doesn't turn out to be the response you need, you must rely entirely on the more laborious B mechanism, which ultimately will give you the required response if that response is in your repertoire.'"

Maier and Schneirla (1942) have also considered this problem in a stimulating paper. They believe that in avoidance train-

ing, the omission of shock changes the procedure in a *qualitative* manner, and state their opinion that two distinct types of learning are successively present, namely, conditioning or "associative learning", working so to speak blindly through contiguity, and "selective learning", related to an effect principle. They regard it as unwarranted to break down this distinction by making much of the similarities between substitutive conditioning and other types of learning, such as trial-and-error or problem-solving. These authors concur with earlier writers (Culler, 1938) that a major function of the unconditioned stimulus is to provide the pattern of response; they differ in that they regard the other half of the duality of conditioning to lie not in a second function of the unconditioned stimulus (to provide motivation), but in a change in the actual nature of the learning process.

But while Maier and Schneirla cogently argue that their theory explains various difficult types of phenomena, it is probably not necessary to follow them in so rigorous a dichotomy. Without at this time making any statement as to the nature of effect reinforcement, it is possible to conceive of the conditioning process as being essentially a unitary one from start to finish, merely by noting that the effect of *avoidance* of shock is similar in results to the effect of shock itself. We are on fairly sure ground here. The facts concerning the irrelevance of what unconditioned stimulus is applied, once conditioning is under way—whether the locus of shock be changed (Brogden, 1940) or food be substituted (Brogden, 1939a)—are well known.

In support of this contention should be cited Mowrer's general theory of anxiety-reduction as a motivating agent, and especially his monograph with Lamoreaux (1942). In this study it is supposed that if, in avoidance conditioning, anxiety-reduction may function as a motive then its greatest effect should take place when the conditioned stimulus disappears as soon as the conditioned response appears, that is, when the conditioned response "turns off" the conditioned stimulus. The reason would be that the temporal gradient of reinforcement is maximal at the time of reinforcement. With three groups, in which the conditioned stimulus was turned off either before, with, or after the appear-

ance of the conditioned response, the results were such as to justify the contention. Mowrer's conclusion is of interest: "... conditioned avoidance response is independently perpetuated, under favorable conditions, by the reinforcement resulting from the anxiety-reduction which accompanies termination of the CS." From observations of his own on animals, the writer is inclined to feel that Mowrer is correct.

By thus bringing the later, precise phase of a conditioning routine under the rubric of direct reinforcement (even though roughly speaking not a stimulus but the absence of a stimulus does the trick) we may well doubt whether there is a sudden change in the dynamics of conditioning at some point where a noxious stimulus is first avoided.

Once it is clear that a situation contains a CS and a UCS, in proper relationship, the matter of adaptation about which Stephens, and Maier and Schneirla concern themselves can readily be explained purely in classical conditioning terms. It is hardly necessary for Maier and Schneirla to explain, for example, the reduction of amplitude of a foot-withdrawal sequence by bringing in a "new" principle of "selective learning", when it may more simply be described as a labile balance between extinction and reconditioning. Limitations of space forbid us here to discuss how effectively "selective learning" may truly be described in conditioning terms.

In summary, the alternatives to a crude sort of stimulus-substitution may be reviewed as follows: The principle of "expectancy", particularly as advanced by Hilgard and Marquis, suffers from the criticisms of Stephens, but must be viewed askance for reasons more fundamental than that it fails to cover *some* expectancies. First, the argument implies that the response is reinforced because the expectancy is confirmed, and also that the increased response shows what was expected. Secondly, it can have little explanatory value with animals. Thirdly, it stands beyond the reach of physiological verification. It is true that Freeman (1939) in a preliminary paper has demonstrated that tonic and postural adjustments (prior to the appearance of the phasic "CR") in a dog can be differentiated when one tone is

followed by one of the following stimuli in conditioning training:

Tone 1 — shock to Right rear paw

Tone 2 — shock to Right or Left rear paw in random order

Tone 3 — shock to Right or Left rear or Right front paw in random order

Tone 4 — shock to any one of the four paws in random order.

In this sense "expectancy" can have immediate and significant meaning; but it is related to the phenomenon of set, rather than constituting an independent principle of reinforcement. In any event, the concept of expectancy should not be used to obviate the need for more adequate explanation.

The second alternative, that of effect, must certainly be a factor. Stated in general terms, it is undoubtedly true that most responses which serve a purpose are retained and refined, most which do not are lost. What this mechanism may be is not here an issue. As applied to conditioning, one can hardly quarrel with the authors cited above who see effect learning wrought into especially the latter phases. It is only necessary to emphasize that no new type of learning is involved in this case; rather that known conditioning mechanisms can well explain the otherwise amazing precision of the CR.

When then of stimulus-substitution? It remains the only good explanation of the appearance of a new CR, except perhaps in those situations where a "spontaneously" emitted response is conditioned. As such the principle is admittedly not adequate to cover the elaboration of conditioned responses as we see them changing in form. In the latter case many variables are operative—variables of fatigue, extinction, inverse conditioning, generalization, irradiation, and a host of others—all perchance affecting the response at one time or other. But for the incursion into the subject's repertory of a new response to a stimulus, we must look to a substitutive process.

This is not to say that the ultimate description of the physiological mechanisms of this shunting will be simple. We know of course that conditioning may occur in simple organisms, and even in the decorticate and spinal dog; yet the latter signifies very little for the moment since at its simplest the complexity of the mammalian cord is such that it could sustain great explana-

tory demands. It is easy to agree with Maier and Schneirla that conditioning may depend not upon sensory-motor connections in the nervous system as has traditionally been thought, but upon sensory-sensory associations (*i.e.*, between centers for CS and UCS). They view the conditioning process not as a shunting but as a progressive extension, or integration, of sensory fields. They base their argument on such evidence as this: that direct shock on the cortex to the motor point of a response cannot serve as a UCS for that response; that only when some sensory component of the UCS is present does conditioning occur. There is some doubt as to this last proposition, it should be added (Loucks and Gantt, 1938). But while the evidence which is mustered for sensory-sensory connection cannot be regarded as crucial, the argument is ingenious and must be borne in mind. In any case, the use of such terms as "integration" by these careful thinkers reveals that although stimulus-substitution is a prime factor in conditioning, it is certainly not the simple "switching" pictured in some descriptions of the past.

Is movement necessary for learning?

No question could be of more theoretical interest than the perplexing one of the role of actual movement in the learning process. Several theories of learning, so-called "peripheral", depend upon an affirmative assertion, opposed by several which espouse a "central" or even "intra-cerebral" view. Until quite lately, nothing definite could be said, but it is now possible to bring some experimental evidence to bear.

First work seemed to indicate that actual response was dispensable, when Crisler showed that dogs treated with atropine so that salivation was prevented, and then given conditioning training, salivated to a CS when the effect of the drug wore off. This result seemed at the time clear-cut. Moreover, Light and Gantt's experiment agreed with this indication, in that a muscle group whose nerve supply had been interrupted, showed, after appropriate training, an appearance of conditioning, subsequent to the nerve regeneration which made the response possible. Both of these experiments have, however, been subjected to

searching criticism. Light and Gantt themselves regard Crisler's work as unclear because his UCR was salivation to morphine. Since salivation in morphine conditioning may arise secondarily, from nausea, Crisler's experiment may be interpreted in these terms.

But it is of considerable importance to note that the experiment of Light and Gantt may be subjected to much the same criticism. Thus, Kellogg and his co-workers (Kellogg, Scott, *et al.*, 1940) have repeated Light and Gantt's study with more extensive notation of bodily activity. They find it very significant that much more happens to a dog when given buzz-shock training than merely learning to lift a leg. It seems that, although flexion muscles were paralyzed, generalized conditioned responses sometimes actually succeeded in removing the paw from the grid. As they put it, the animal "becomes conditioned 'all over' to the buzz stimulus. The flexion of the leg is but an element of what ultimately becomes a well-integrated pattern of response. When experimental circumstances permit, it fits into the rest of the picture."

These studies then cannot be taken as unequivocal support of the theory that movement may be dispensed with in conditioning.

The early statement of Harlow and Stagner that movement *is* necessary for learning has received recent experimental support—though their experimental work, with curare, perhaps needs re-examination in view of Girden and Culler's description (1937) of the peculiar effects of this drug on conditioning. The latter workers found that a response conditioned under curare could not be elicited in the normal state, and vice versa. Harlow and Stagner's animals could, therefore, hardly furnish a crucial test of the motor theory. On the other hand, Harlow (1940) found six cases in which cats showed conditioned responses after curare had worn off, even though the response had not appeared in those animals during conditioning training under curare. It should be said that only two of these were skeletal CR's, the others being pupillary.

Girden has amplified the data on this problem most recently (1943). He confirms Harlow's observation that pupillary re-

sponses can be formed under curare (or erythroidine, a drug similar in pharmacological effect to curare, and more stable in its action), but finds that, when given prolonged conditioning training under a deep dosage, no striate response whatever appears when the animal makes a partial recovery. At the partial recovery stage, the control was instituted of demonstrating that conditioning training was now capable of producing genuine conditioned responses. That the failure of conditioning under deep dosage was not due to depression of the central nervous system was shown both by recordings of autonomic activity which *did* show conditioning at that stage, and by an ingenious control, suggested by an experiment of Harlow and Settlage (1939), consisting of the ligation, during deep dosage, of a single muscle. This muscle, deprived of its blood supply and consequently unaffected by erythroidine, remained responsive during part of the deep drug stage: convincing proof that the nerve supply to the striate muscles was adequate to subserve conditioning. Yet no evidence of conditioning in these muscles could be ascertained when the drug had worn off sufficiently to permit ready movements.

The writer is in agreement with the position taken by Girden that these results support the response-theory of conditioning learning. Only one point need be mentioned: in view of his insistence on the principle of "dissociation" as a mechanism separating the curare from the normal state (Girden, 1940), he will sooner or later be pressed to show that a similar phenomenon does not exist for striate muscles between the deep vs. the mild drug stage. If such "dissociation" occurs, no transfer from one stage to the other could be expected, and his test for conditioning during the "mild" drug stage will prove no more valid than Girden and Culler have shown the test in the normal stage to be.

Certain other experiments will be thought of in connection with this problem, notably those on "sensory" conditioning, and on conditioning the alpha rhythm of the electrical activity of the cerebral cortex. In these experiments actual movement is by no means ruled out, but certainly its role is minimal; nevertheless results in general are positive. Ellson (1942), for exam-

ple, has produced hallucinatory synesthesia, and shown a possible reason for E. L. Kelley's failure to confirm some of Ellson's earlier work (1941). Also, it now seems that the blocking of the cortical alpha rhythm may be effected through conditioning (Travis and Egan, 1938; Knott and Henry, 1941; Jasper and Shagass, 1941; Shagass, 1941, 1942; Shagass and Johnson, 1943). While it is probably true that the alpha rhythm itself is exclusively a neural phenomenon, it may be that its "blocking" by a light is the result of some effector response to that light. If so, its conditioning would have a straightforward explanation. However, so far this has not been shown.

What is the relation of conditioning to processes simultaneously affecting the CS?

Recently, several interesting facts have appeared which show that it is not enough to refer the appearance and characteristics of a conditioned response solely to the process of conditioning. It has long been known that the CR is dependent in part upon the particular strength of stimuli used, the physiological condition of the subject as to hunger, drugs, and the like. But quite lately it is being recognized that there are processes set up by and inherent in the conditioning procedure which directly affect strength of response, yet are no part of conditioning *per se*. The most striking of these is pseudo-conditioning (Grether, 1938), related to the presentation of the UCS alone. Subsequently, response to a formerly neutral stimulus may be present. Some recent papers expressly concerned with this phenomenon are those of Grant (1943a, b), Grant and Dittmer (1940), Grant and Hilgard (1940), Grant and Meyer (1941), Harlow (1939), Harlow and Bromer (1942), Harlow and Toltzien (1940), Harris (1941, 1942a), Sgonina (1939), and Wickens and Wickens (1942). These have been summarized and discussed elsewhere (Harris, 1943). It looks increasingly as though pseudo-conditioning, at first thought nearly identical with the "dominance" phenomenon of Ukhtomski and Ufland, may better be explained as a special type of conditioning. At least Wickens and Wickens present some evidence to this effect.

Some apparently anomalous data of Hilgard and Grant, who showed that two groups, one of which simply sat quietly during a period comparable to that of another group receiving ordinary conditioning training, were indistinguishable during "extinction" tests, have not been confirmed in animals (Harris, 1943) and may presumably be related to the complex attitudes of human beings subjected to a laboratory situation.

Of other non-associative factors, habituation has come in for brief attention. Razran (1939a) especially has emphasized the role it may play in extinction. Harris has shown that habituation may affect not only the CR but also the UCR during actual conditioning (1942b), and that habituation to a stimulus will retard later conditioning to that stimulus (1943).

A general conclusion to be reached is that while non-associative factors may and do complicate conditioning procedure, only a moderate effort is required so to control them that conditioning results may confidently be interpreted.

*What does conditioning have to say about the dynamics
of human behavior?*

In considering human behavior as we see it under non-laboratory conditions, one is struck with the fact that it often seems far removed from the fundamental urges to which all of us are subject. Conditioning theory has made bold to approach this major problem armed with the facts of higher-order conditioning, in which a CS of the first order becomes the UCS of the second order, and so on. In successive steps one may thus divorce oneself of the necessity of providing, so to speak, primary reinforcement for every failure to make an appropriate response. But it has long been known that with animals and with children it is impossible to get biologically satisfactory results beyond a bare three or four orders. Does this mean that conditioning cannot help explain the sinuosities of human motivation? It has been suggested that the basis of this complexity was partly revealed by an experiment of Finch and Culler who showed that shock to the thorax, if applied for failure to respond at any order, would permit the orders to mount indefinitely. Complex so-

cial behavior, therefore, if elaborated in something akin to higher order sequence with the primary reinforcement of food, could be maintained in the absence of any food if only some other reinforcement could be supplied. This conclusion may need to be modified somewhat by a later experiment of Brogden (1939a) in which food was given for correct responses in higher-order, although the primary conditioning had been established to paw-shock. Here, only four out of ten dogs could go to third- and fourth-order. We do not, of course, suggest that this implies that man is limited to so few orders.

Some very good thinkers believe that we are not justified to regard human behavior as being far removed from fundamental physiological motivation, and profess to show that most, if not all, behavior is grounded almost directly on bodily needs. There are, however, a few recent experiments which make possible a new approach to the question.

It is possible that the conditioned response comes to acquire considerable motivating value in and of itself. Harlow (1937) first showed this by removing the force of an unconditioned stimulus with the technique of habituation. In spite of the truly insignificant role to which this procedure relegated the CS, however, Harlow's monkeys were still responding with all their accustomed wont and vigor to the CS, even after 200 trials of experimental extinction. But an even more striking proof of the potency of the CS was provided when he presented the now-neutral (former) UCS as a CS *of the second order*; the value of the CS was apparent when this clever reversal was accomplished without difficulty so that the former UCS was reinstated in the monkey's hierarchy of dominant response.

Another example of how a CR may become biologically meaningful as a motivating agent is provided by Eccher and Culler (1941). Here the value of a CS was shown to increase, when used as a UCS in the second order, over its value even with continued primary reinforcement. It was stated that this experiment suggests certain types of human behavior: "Since the bell is at first neutral and acquires incentive-value through its association with shock, it inevitably becomes a *symbol* for punishment-

by-shock. In man it is notorious that more extensive and intensive emotional behavior may attach to a symbol than to the entity symbolized . . . it may be that a mild phobia is developing for the bell; in which case the facilitation may be due not alone to the tone or the light which is being conditioned but likewise to the organic (autonomic-thalamic) involvement underlying the fear."

Still another example of the effect which a CS may come to have is the fact that it may exert a facilitative effect on the UCR (Harris, 1942b), this effect increasing parallel with the strength of the CR itself.

It is more than likely that the foregoing experiments bear directly on the perplexing question of what Allport has called the "functional autonomy of motives". Woodworth also has recognized the problem with his statement that mechanisms become drives. On the surface it often does look as though responses came to be perpetuated in and of themselves. On the other hand, no special force within an act need be postulated to explain the continuation of that act, if already known or reasonable mechanisms can be invoked. Certainly conditioning provides many ways in which a response can be maintained. The above-cited experiments make it clear that powerful emotional reinforcements can be provided, for one thing; but other mechanisms tend likewise toward the same result.

In the first place, the actual stimuli effective in producing a response are often very imperfectly known. For example, the suggestion has been made that not the "conditioned stimulus" as naively viewed by the experimenter, but *any* change-in-environment may control the response (Wickens and Wickens, 1940). Also in point is an experiment by Gantt (1940b). A bubbling sound and a metronome were alternated as CS, reinforced by 2 gms. and 12 gms. of food respectively. But when the metronome was given *alone* in a series, the salivation fluctuated as before. (This result was checked for temporal conditioning.) Other examples could be cited. Thus, responses termed "autonomous" may in reality be directed by external conditions hidden from the experimenter.

In the second place, there exists the related fact that as conditioning training progresses, more and more stimuli become more and more capable of calling forth a response. This seems to be the explanation of Brogden's interesting experiment (1942a) in which dogs, even though food-satiated, still exhibited conditioned responses in the training stock. In connection with this experiment we have the demonstration by Razran (1938h) that extinction is more difficult with multiple than with single stimuli. In Brogden's experiment too, then, any "autonomy" is seen to be illusory.

In the third place, it is true that resistance to extinction increases with number of reinforcements (for a recent quantitative statement see Williams, 1938) or, as Humphreys (1943) has shown, with number of practice trials. Hence with very great strength of conditioning, response strength may remain high for some time.

Lastly, the phenomenon of secondary reward may help explain the perpetuation of acts from which the fundamental reinforcement has been removed. For some years it has been the practice in the Rochester laboratory to provide a buzzer after the presentation of the CS and concurrent with the shock. This buzzer, in the absence of shock, aids materially in maintaining a CR. Bugelski (1938) and Humphreys (1943a) have confirmed this in specific experiments. Also pertinent is the extensive literature on token-rewards, which limitations of space forbid us to discuss here.

We see then that far from retreating in the face of the complexity of human motivation, conditioning theory is able to make some direct and valuable statements concerning the dynamics of behavior, statements based moreover on experimental evidence.

*What role does conditioning play in normal human
adult behavior?*

In spite of the fact that morphological development in man should perhaps lead one to expect there the widest use of conditioning learning, it is notorious that man is one of the hardest species to condition. Some responses which are admirable indi-

cators of conditioning in animals, for example, salivation, seem exceedingly difficult to control with human subjects. Although with human salivation several men (Shastin, 1938; Razran, 1939-1940) have performed reliable experiments, others (Jones, 1939) attempting to duplicate their techniques have had little or no success. But even with responses generally successful, a high proportion of studies reports that some subjects did not show any evidence of conditioning. Again, some have contended that so simple a phenomenon as conditioning cannot, by reason of that very simplicity, be a major factor in the complexities of human behavior.

These facts among others have led some writers to inquire just what role conditioning does play in the behavior of normal human adults, whether it is in truth only an artificially isolated segment of behavior with little biological significance for human psychology. It is evident that a satisfactory answer to this question must be furnished in order to help justify the labor spent in conditioning experiments, as well as to warrant the elaborate explanations of human behavior on the basis of principles known to be operative in laboratory situations.

In formulating what we regard as adequate justification for applying conditioning theory to everyday behavior, we are aided by a picture of conditioning much more comprehensive than could have been delineated a few years ago. This has been made possible by several important studies on generalization, compound conditioning, patterning, transposition, semantics, and related principles concerning the CS, and on various types of plasticity encountered in the CR. This evidence clearly indicates that human conditioning does not consist of immutable responses to generically static stimuli, but on the contrary is exceedingly labile both as to adequacy of stimulus and appropriateness of response.

The question of how one stimulus is or comes to be biologically equivalent to another has been considered most carefully by Hull (1939). In addition to the factors of partial physical identity and of "primary" generalization as described by Pavlov and latterly by Hovland, a third factor, "secondary" generalization, is introduced. This factor operates through a response

aroused by either of two stimuli. If now one of these stimuli is made a CS, the other will also acquire some potency through an intermediary response. Hull's theory on this point, then, provides at least one reasonable supposition as to how the simplest conditioning process may come to have wide-spread consequences.

Psychologists are greatly indebted to Razran for his excellent summaries of Russian experiments; this is outstandingly true in the case of conditioning with compound stimuli (Razran, 1939d). Previous to his review we had only one or two non-Russian papers on conditioning with multiple stimuli; but since then a number of studies have been published which tend to show that such conditioning exhibits different characteristics, if it does not indeed follow different laws, from single-CS conditioning. Thus Miller (1939) found a double CS to result in more rapid conditioning than either component alone. Again, Menzies (1941) found that a dual CS was effective, even though neither component sufficed of itself. Razran himself has led in this problem, insisting that the laws of "configural" conditioning are somewhat different from ordinary conditioning with single stimuli. Inasmuch as conditioning probably never occurs to a "single" stimulus, it may be that Razran's fundamental argument loses some of its force; nevertheless he has provided considerable evidence distinguishing the two general situations (1938 b, 1939 d-h, 1940 a). He has made the basic finding that patterning of the CS does occur in man, successive red-green or green-red flashes of lights being more effective than the simpler red-red or green-green patterns. In some cases of pattern conditioning, there may be no response whatever to the components ("supra-summation"). This patterning is subject to attitudinal modification, it emerges clearest when the components are more unlike, and is more resistant to extinction and forgetting. One of his more important findings was that in transferring from one stimulus or one pattern to another, the results in the two cases were so different that he felt it necessary to speak of generalization in the former case, but of transposition in the latter.

On the other hand, Hull and his students cannot find any overwhelming evidence that presenting a patterned CS introduces

new principles. With the human GSR Hull did not find much patterning even with differential reinforcement of pattern and component (1940). It was admitted that responses of striate muscles might show more. With dogs Woodbury obtained similar results (1943) and his conclusion with which Hull presumably agrees, is that so-called pattern conditioning is related systematically to basic properties of conditioning and does not represent qualitatively different material.

That a very high degree of patterning indeed exists in man, there can be no doubt. To take a simple case, Keller (1943) found generalization from the picture of one hat to another hat quite different in appearance. Although Keller did not find transfer to the word "hat", Reiss (1940) had found distinct transfer from words to words. It is of the utmost importance to note that transfer was more to synonyms than to homophones. Obviously here patterning is of the highest order. More detailed study of this semantic transposition (in opposition to a phonetographic process) is given by Razran (1939 b, c), opening up the vast field of meaning to a conditioning approach.

On the response side, likewise, sufficient fluidity is found to enable conditioning theory to handle relatively complex data. With the human adult in this matter it is convenient, if not necessary, to speak of response not in terms of muscular movements but in terms of actions—when a human subject is conditioned to lift his finger from an electrode it is customary to say, not that certain muscles contracted, but that the finger *was removed* (see Guthrie, 1940). It is immaterial for the present discussion how this purposive characteristic intrudes into behavior. What we are struck with is the multiplicity of response avenues by which a result is reached. It appears clearly with dogs. One thinks of Bekhterev's dog which raised the opposite leg when the experimental leg was tied down. One thinks of Konorski and Miller's "motor generalization" phenomenon. A dog was trained to lift his hind leg to a piano note, and to raise his foreleg in response to a light. If the piano note were to be sounded, a recording device was attached to the appropriate leg, and so for the light. After conditioning, what was the experimenters' amaze-

ment to find that, on control experiments, the animal would raise the leg to which was attached the recording device, irrespective of what "CS" was presented. Further, when the device was attached to either of the two "neutral" legs neither tone nor light elicited any movement whatsoever from any of the four legs. True in dogs, what complexities of behavior could conditioning explain in man!

In the human adult, response generalization has been shown no more clearly than by Wickens (1938, 1939, 1943 a, b), who demonstrated that after conditioning finger-withdrawal, turning the hand over (so that *antagonistic* muscles were effective in avoiding shock) in no way disconcerted the subjects. Furthermore, such transferred responses suffered no decrement in strength.

Kellogg and Walker (1938), Kellogg (1939), and Pronko and Kellogg (1942) have shown that in the dog this sort of transfer follows rather definite neurological lines, but no comparable data have come from the human species.

Together with data indicating the breadth of CS and plasticity of CR, should be juxtaposed statements showing that almost every sort of variable of the organism may profoundly influence human conditioning. Wickens (1940) especially has taken pains to contravert the notion that the conditioning paradigm represents an isolated bit of behavior. He clearly documents from recent experiments the assertions that conditioning is vitally affected by attitudes (Cole, 1939; Grant, 1939; Hilgard and Humphreys, 1938; Miller, 1939), by other conditioned responses, by organic and postural states of the subjects, and by perceptual factors; and says that since "all have an undeniable influence on the quantitative and qualitative nature of the conditioned response, it seems impossible to consider the conditioned response and the organismic points of view as discordant."

It must be emphasized that Wickens' listing of possible factors influencing the conditioned response in no way indicates an explanatory sterility of conditioning theory but on the contrary suggests ways in which the technique of conditioning may be used in an experimental attack on just those factors mentioned.

In summary, it seems to the writer that statements purporting to restrict conditioning theory from considering complex human behavior are based upon a superficial envisagement of the conditioning process, and may result in the neglect of a powerful quantifying tool.

Experimental neurosis produced by conditioning.

Of the manifold applications to which conditioning has been and is being put—experimental neurology, clinical neurology, sensory physiology, infant psychiatry, to name only a few—space prevents us from discussing more than one. We choose experimental neurosis as one of the liveliest fields.

In recent years two or three conditioning laboratories in this country have been working extensively with abnormal behavior developed in conditioning procedures, just as Pavlov latterly turned his energies to a study of the same subject. In the case of Liddell this interest goes back nearly to the beginning of his work; nevertheless since his most significant approach to the study of experimental neurosis demanded observations on single animals extending over a period of years, it has been only lately that his theories have become generally public (1942).

In the first place, Liddell considers two most important factors to be the "monotonous and unsatisfying repetitiveness" of the presentation of stimuli, combined with the self-restraint demanded in the training stock. So far as the behavioral picture of a long-trained animal is concerned, Liddell says, "the reinforcing agents, shock or food, cannot be thought of as important goals polarizing the animal's behavior. Observations of the responses to these routine unconditioned stimuli give us scarcely more insight into the consequence of a long-continued regimen of training in the laboratory than do punctuation marks in helping us to understand the written page." From this point of view the production of experimental neurosis need not be a sudden onset, though the underlying mechanism may be nonetheless a traumatic one. The consequences of months and years of ordinary conditioning training may then, in some animals, lead progressively to what Liddell calls the "pathological terminus", or

experimental neurosis. This should have been explained to the farmer who said his wife went insane "even though she had not been out of the kitchen in forty years". Liddell's observations on the subject are supported by extensive case histories of neurotic animals, by Anderson and Parmenter (1941) from Liddell's laboratory. Also to be mentioned is some previously unpublished work of Liddell and Sutherland on the effect of progressive restraint during conditioning. A pig was trained on one day to lift a box lid for food upon hearing a high tone, and on alternate days to lift the leg at a low tone. This was readily learned. At this point restriction was introduced in the form of shock if the pig touched the surrounding fence at any time, or touched the food box at any time other than during the ten-second period of high (food) tone. Thereupon abnormalities developed. Not only did the pig now touch the box only when the food could be heard to drop into it (*i.e.*, conditioning was lost), but reconditioning, with no shock in any form, was now ineffective. Actually the abnormality became progressively worse; at first the animal might attack the box, but later avoided it altogether, even when the cover was open and food could be seen falling into the box after every trial. Only when the experimenter went to release the animal for the day would it rouse and rapidly eat what had accumulated.

Some of Liddell's more important observations, recently emphasized, concern the disturbance of social relations as a result of conditioning training. The animal's attitude toward the experimenter proceeds "first in the direction of dependence and solicitation, but later toward avoidance and hostility." He is here speaking of dogs given simple conditioning for long periods.

James (1943) has evolved a novel method for inducing experimental neurosis in the dog. In forcing them to work strenuously to accomplish a conditioned avoidance to shock, he noticed from records of leg activity, breathing, head movements, and heart rate, abnormalities which were typical of dogs made neurotic by the difficult-discrimination technique. When the work requirement became great, however, he had to increase the shock a good deal to make the animal get off the grid. Hence it does

not appear that he has separated the variables of work-requirement and of shock in explaining this phenomenon.

Gantt has likewise turned the attention of the Pavlovian Laboratory at Johns Hopkins to a consideration of experimental neurosis developed by the difficult-discrimination method (1942). He has extended his observations to include much of the systemic activity—respiratory, cardiac, gastro-intestinal, genito-urinary, among others—of the neurotic animal, as well as data on general activity, constitution, and social relationships, and has furthermore made some attempts at therapy.

The most important generalization which the work of Gantt permits is that it definitely is possible to foresee a "breakdown", before any overt indication occurs, by the close observation of covert responses. He presents norms for salivation (1938) and for heart-rate (Gantt and Hoffman, 1940), deviations from which are said to foreshadow abnormalcy. While the meaning of these observations for human behavior is by no means crystal clear, certain parallels make it impossible to refrain from speculation. Gantt finds, for example, that in a dog with a two-year history of conflict, frequent micturition to the point of pollakiuria (30 times in an hour) was present. He comments, "... one of the most neurotic of our dogs began urinating frequently anywhere and everywhere in the environment of conflict, and even aggressively . . . One might easily draw a parallel here to the enuresis of nervous children." Another illustration is provided by Gantt and Muncie (1941) on the extent of the difference between normal and neurotic dogs outside the conflict situation. It was found that normal dogs, placed in adjacent cages, show parallel fluctuations in general activity, while neurotics show no such correspondence to their neighbor.

As for successful therapy, Gantt found the best treatment was a rest in the country, the more prolonged the better. After 18 months change of environment one dog became almost normal in behavior, while upon return to the laboratory his old symptoms, though recurrent, were lessened in degree. Habituation to the conflict situation had no effect, nor even complete rest in the living quarters of the laboratory. Food given in the conflict

situation reduced symptoms only during feeding.

As to therapy, a few animal studies have been done in imitation of the work in human cases with shock therapy for psychosis. Rose, *et al.* (1938), for example, used insulin shock, Gellhorn electric shock, and Rosen and Gantt (1942) metrazol. It is too soon to draw really instructive parallels, though encouraging results are sometimes obtained.

Of other treatments of the topic should be mentioned that of Beier (1940). He succeeded in establishing conditioned cardio-vascular responses to indifferent stimuli in man; what is valuable are his suggestions for the conditioning treatment of the cardiac neuroses so prevalent in high-speed civilizations. Lastly, we may speak of the successful conditioning treatment of enuresis (Morgan and Witmer, 1939; Deacon, 1939) following Mowrer's bed-pad and gong technique—though in the case of enuresis should be considered the cautions of psychiatrists (Michaels, 1939) against neglecting other aspects of the psychiatry of the child, notably the child-parent relationship.

In closing this review, the author makes no apology for raising these issues at this time. It is even possible that matters such as we have been discussing will ultimately prove of more import than questions pertaining directly to the battles in which mankind is now engaged.

REFERENCES

- ANDERSON, O. D., and PARMENTER, R. (1941) A long-term study of the experimental neurosis in the sheep and dog, with nine case histories. *Psychosom. Med. Monogr.*, 2, No. 3, 4.
- BEIER, D. C. (1940) Conditioned cardiovascular responses and suggestions for the treatment of cardiac neuroses. *J. exp. Psych.*, 26, 311-321.
- BROGDEN, W. J. (1939a) Unconditioned stimulus-substitution in the conditioning process. *Amer. J. Psych.*, 52, 46-55.
- BROGDEN, W. J. (1939b) Higher order conditioning. *Amer. J. Psych.*, 52, 579-591.
- BROGDEN, W. J. (1940) Conditioned flexion responses in dogs re-established and maintained with change of locus in the application of the unconditioned stimulus. *J. exp. Psych.*, 27, 583-600.
- BROGDEN, W. J. (1942) Non-alimentary components in the food-reinforcement of conditioned forelimb-flexion in food-satiated dogs. *J. exp. Psych.*, 30, 326-335.
- BUGELSKI, P. (1938) Extinction with and without sub-goal reinforcement. *J. comp. Psych.*, 26, 121-134.

- COLE, L. E. (1939) A comparison of the factors of practice and knowledge of experimental procedure in conditioning the eyelid response of human subjects. *J. gen. Psych.*, 20, 349-373.
- CULLER, E. (1938) Recent advances in some concepts of conditioning. *Psych. Rev.*, 45, 134-153.
- CULLER, E., FINCH, G., GIRDEN, E., and BROGDEN, W. J. (1935) Measurements of auditory acuity by the conditioned-response technique. *J. gen. Psych.*, 12, 223-227.
- DEACON, J. R. (1939) The conditioned habit treatment of nocturnal enuretics. *Proc. Amer. Ass. ment. Def.*, 44, No. 2, 133-138.
- ECCHER, W., and CULLER, E. (1941) Reciprocal facilitation of the conditioned and conditioning mechanisms. *J. comp. Psych.*, 31, 223-231.
- ELLSON, D. G. (1941) Hallucinations produced by sensory conditioning. *J. exp. Psych.*, 28, 1-20.
- ELLSON, D. G. (1942) Critical conditions influencing sensory conditioning. *J. exp. Psych.*, 31, 333-338.
- FREEMAN, G. L. (1939) A preliminary study of the role of postural adjustment in conditioning. *Amer. J. Psych.*, 52, 89-94.
- GANTT, W. H. (1938) The nervous secretion of saliva: the relation of the conditioned reflex to the intensity of the unconditioned stimulus. *Amer. J. Physiol.*, 123, 74-75.
- GANTT, W. H. (1940) The role of the isolated conditioned stimulus in the integrated response pattern, and of the relation of pattern changes to psychopathology. *J. gener. Psych.*, 23, 3-16.
- GANTT, W. H. (1942) The origin and development of nervous disturbances experimentally produced. *Amer. J. Psychiatr.*, 98, 475-481.
- GANTT, W. H., and HOFFMAN (1940) Conditioned cardio-respiratory changes accompanying conditioned food reflexes. *Amer. J. Physiol.*, 129, 360-361.
- GANTT, W. H., and MUNCIE, W. (1941) Rhythmic variations of muscular activity in normal and neurotic dogs correlated with secretion and with conditioned reflexes. *Amer. J. Physiol.*, 133, P287.
- GIRDEN, E. (1940) Cerebral mechanisms of conditioning under curare. *Amer. J. Psych.*, 53, 397-406.
- GIRDEN, E. (1943) Role of the response mechanism in learning and in "excited emotion". *Amer. J. Psych.*, 56, 1-21.
- GIRDEN, E., and CULLER, E. (1937) Conditioned responses in curarized striate muscle in dogs. *J. comp. Psych.*, 23, 261-274.
- GRANT, D. A. (1939) The influence of attitudes on the conditioned eyelid response. *J. exp. Psych.*, 25, 333-346.
- GRANT, D. A. (1943a) The pseudo-conditioned eyelid response. *J. exp. Psych.*, 32, 139-140.
- GRANT, D. A. (1943b) Sensitization and association in eyelid conditioning. *J. exp. Psych.*, 32, 201-212.
- GRANT, D. A., and DITTMER, D. G. (1940) A tactile generalization gradient for a pseudo-conditioned response. *J. exp. Psych.*, 26, 404-412.
- GRANT, D. A., and HILGARD, E. R. (1940) Sensitization as a supplement to association in eyelid conditioning. *Psych. Bull.*, 37, 478-479.
- GRANT, D. A., and MEYER, H. I. (1941) The formation of generalized response sets during repeated electric shock stimulation. *J. gener. Psych.*, 24, 21-38.
- GRETHER, W. F. Pseudo-conditioning without paired stimulation encountered in attempted backward conditioning. *J. comp. Psych.*, 25, 91-96.
- GUTHRIE, E. R. (1935) *The Psychology of Learning*, N. Y.: Harpers, p. 258.

- GUTHRIE, E. R. (1940) Association and the law of effect *Psych. Rev.*, 47, 127-127-148.
- GUTHRIE, E. R. (1942a) The principle of associative learning In Clarke, E. P., and Nahm, M. C., *Philosophical Essays in Honor of Edgar Arthur Singer, Jr.* Phila. University of Pennsylvania Press. Pp 100-114.
- GUTHRIE, E. R. (1942b) Conditioning A Theory of Learning in Terms of Stimulus, Response, and Association. *Yearb. nat. Soc. Stud. Educ.*, 41, Part 2, 17-60 Bloomington, Ill: Public School Publishing Co
- HARLOW, H. F. (1937) Experimental analysis of the role of the original stimulus in conditioned responses in monkeys. *Psych. Record*, 1, 62-68.
- HARLOW, H. R. (1939) Forward conditioning, backward conditioning, and pseudo-conditioning in the goldfish *J. genet. Psych.*, 55, 49-58.
- HARLOW, H. F. (1940) The effects of incomplete curare paralysis upon formation and elicitation of conditioned responses in cats. *J. gener. Psych.*, 56, 273-282.
- HARLOW, H. F., and BROMER, J. A. (1942) Acquisition of new responses during inactivation of the motor, premotor, and somesthetic cortex in the monkey *J. gener. Psych.*, 26, 299-313
- HARLOW, H. F., and SETTLAGE, P. (1939) The effect of curarization of the forepart of the body upon the retention of conditioned responses in cats. *J. comp Psych.*, 27, 45-48.
- HARLOW, H. F., and TOLTZIEN, F. (1940) Formation of pseudo-conditioned responses in the cat *J. gener Psych.*, 23, 367-375
- HARRIS, J. D. (1941) Forward conditioning, backward conditioning, pseudo-conditioning, and adaptation to the conditioned stimulus. *J. exp Psych.*, 28, 491-502
- HARRIS, J. D. (1942a) A gradient of pseudo-conditioning. *Psych. Bull.*, 39, 479.
- HARRIS, J. D. (1942b) Facilitation of the unconditioned responses by the conditioned stimulus in buzzer-shock conditioning of rats. *Psych. Bull.*, 39, 598.
- HARRIS, J. D. (1943) Studies on non-associative factors inherent in conditioning *Comp. Psych. Monogr.*, 18, No. 93, p. 74.
- HILGARD, E. R., and HUMPHREYS, L. G. (1938) The effect of supporting and antagonistic voluntary instruction on conditioned discrimination. *J. exp Psych.*, 22, 291-304.
- HILGARD, E. R., and MARQUIS, D. G. (1940) *Conditioning and Learning*. N. Y.: Appleton Century.
- HULL, C. L. (1939) The problem of stimulus equivalence in behavior theory. *Psych. Rev.*, 46, 9-30.
- HULL, C. L. (1940) Explorations in the patterning of stimuli conditioned to the G. S. R. *J. exp. Psych.*, 27, 95-110.
- HUMPHREYS, L. G. (1943) The strength of a Thorndikian response as a function of the number of previous trials. *J. comp. Psych.*, 35, 101-110.
- JAMES, W. T. (1943) The formation of neurosis in dogs by increasing the energy requirement of a conditioned avoiding response. *J. comp. Psych.*, 36, 109-124.
- JASPER, H., and SHAGASS, C. (1941) Conditioning of the occipital alpha rhythm in man. *J. exp. Psych.*, 28, 373-388.
- JONES, L. F. (1939) A study of human salivary conditioning. *J. exp. Psych.*, 24, 305-317.
- KELLER, M. (1943) Mediated generalization: the generalization of a conditioned galvanic skin response established to a pictured object. *Amer. J. Psych.*, 55, 438-448.

- KELLOGG, W. N. (1939) The relationship between ambiguous conditioning and experimental extinction in dogs. A follow-up report. *J. comp. Psych.*, 27, 283-287.
- KELLOGG, W. N., SCOTT, V. B., DAVIS, R. C., and WOLF, I. S. Is movement necessary for learning? An experimental test of the motor theory of conditioning. *J. comp. Psych.*, 29, 43-73.
- KELLOGG, W. N., and WALKER, E. L. "Ambiguous conditioning", a phenomenon of bilateral transfer. *J. comp. Psych.*, 26, 63-78.
- KNOTT, J. R., and HENRY, C. E. (1941) The conditioning of the blocking of the alpha rhythm of the human electroencephalogram. *J. exp. Psych.*, 28, 134-144.
- LIDDELL, H. S. (1942) The alteration of instinctual processes through the influence of conditioned reflexes. *Psychosom. Med.*, 4, 390-395.
- LOUCKS, R. B., and GANTT, W. H. The conditioning of striped muscle responses based upon faradic stimulation of dorsal roots and dorsal columns of the spinal cord. *J. comp. Psych.*, 25, 415-426.
- MAIER, N. R. F., and SCHNEIRLA, T. A. (1942) Mechanisms in conditioning. *Psych. Rev.*, 49, 117-135.
- MENZIES, R. (1941) Further studies of conditioned vasomotor responses in human subjects. *J. exp. Psych.*, 29, 457-482.
- MICHAELS, J. J. Enuresis—a method for its study and treatment. O. H. Mowrer and Willie Mae Mowrer. A critique. *Amer. J. Orthopsychiat.*, 9, 629-635.
- MILLER, J. (1939a) The rate of conditioning human subjects to single and multiple conditioned stimuli. *J. gener. Psych.*, 20, 399-408.
- MILLER, J. (1939b) The effect of inhibitory and facilitatory attitudes on eyelid conditioning. *Psych. Bull.*, 36, 577-578.
- MORGAN, J. J. B., and WITMER, F. J. (1939) The treatment of enuresis by the conditioned reaction technique. *J. genet. Psych.*, 55, 59-65.
- MOWRER, O. H., and LAMOREAUX, R. R. Avoidance conditioning and signal duration—a study of secondary motivation and reward. *Psych. Monogr.*, 54, No. 5, p. 34.
- MUNN, N. L. (1939) The relative effectiveness of two conditioning procedures. *J. gener. Psych.*, 21, 119-136.
- PRONKO, N. H., and KELLOGG, W. N. (1942) Reflex mechanisms and the ease of conditioning. *Amer. J. Psych.*, 55, 371-384.
- RAZRAN, G. H. S. (1939a) The nature of the extinctive process. *Psych. Rev.*, 46, 246-297.
- RAZRAN, G. H. S. (1938) Transposition of relational responses and generalization of conditioned responses. *Psych. Rev.*, 45, 532-538.
- RAZRAN, G. H. S. (1939b) Semantic, syntactic, and phonetographic generalization of verbal conditioning. *Psych. Bull.*, 36, 578.
- RAZRAN, G. H. S. (1939c) A quantitative study of meaning by a conditioned salivary technique. *Science*, 90, 89-90.
- RAZRAN, G. H. S. (1939d) Studies in configural conditioning. I. Historical and preliminary experimentation. *J. gener. Psych.*, 21, 307-330.
- RAZRAN, G. H. S. (1939e) Studies, etc. II. The effect of the subject's attitudes and of task-sets upon configural conditioning. *J. exp. Psych.*, 24, 95-105.
- RAZRAN, G. H. S. (1939f) Studies, etc.: III. The factors of similarity, proximity, and contiguity in configural conditioning. *J. exp. Psych.*, 24, 202-210.
- RAZRAN, G. H. S. (1939g) Studies, etc.: IV. Gestalt organization and configural conditioning. *J. Psych.*, 7, 3-16.
- RAZRAN, G. H. S. (1939h) Studies, etc.: VI. Comparative extinction and forgetting of pattern and of single-stimulus conditioning. *J. exp. Psych.*, 24, 432-438.

- RAZRAN, G. H. S. (1940) Studies, etc.: V. Generalization and transposition. *J. genet. Psych.*, 56, 3-11.
- REISS, B. F. (1940) Semantic conditioning involving the galvanic skin reflex. *J. exp. Psych.*, 26, 238-240.
- ROSE, J. A., TANTON-POTTBERG, A., and ANDERSON, O. D. (1938) Effects of insulin shock on behavior and conditioned reflex action in the well-trained sheep. *Proc. Soc. Exp. Biol.*, N. Y., 38, 653-655.
- ROSEN, V. H., and GANTT, W. H. (1942) The effect of metrazol convulsions on conditioned reflex training in dogs. *Fed. Proc. Amer. Soc. exp. Biol.*, 1, No. 1, Part II, 74-75.
- SCHLOSBERG, H. (1937) Success and the laws of conditioning. *Psych. Rev.*, 44, 379-394.
- SGONINA, K. (1939) Vergleichende Untersuchungen über die Sensibilisierung und den bedingten Reflex. *Z. Tierpsych.*, 3, 224-247.
- SHAGASS, C. (1941) Conditioning the electrical response of the visual cortex in the cat. Unpublished MS. thesis, University of Rochester, Rochester, N. Y.
- SHAGASS, C. (1942) Conditioning the human occipital alpha rhythm to a voluntary stimulus. A quantitative study. *J. exp. Psych.*, 31, 367-379.
- SHAGASS, C., and JOHNSON, E. P. (1943) The course of acquisition of a conditioned response of the occipital alpha rhythm. *J. exp. Psych.*, 33, 201-209.
- SHASTIN, N. R. (1938) The methodology of studying conditioned reflexes in children. *Fiziol. Zh. S. S. S. R.*, 24, 1055-1062.
- STEPHENS, J. M. (1942) Expectancy vs. effect-substitution as a general principle of reinforcement. *Psych. Rev.*, 49, 102-116.
- TRAVIS, L. E., and EGAN, J. P. (1938) Conditioning of the electrical response of the cortex. *J. exp. Psych.*, 22, 524-531.
- WICKENS, D. D. (1938) The transference of conditioned excitation and conditioned inhibition from one muscle group to the antagonistic muscle group. *J. exp. Psych.*, 22, 101-123.
- WICKENS, D. D. (1939) The simultaneous transference of conditioned excitation and conditioned inhibition. *J. exp. Psych.*, 24, 332-338.
- WICKENS, D. D. (1940) Conditioned response data and the holistic point of view. *Psych. Rev.*, 47, 155-169.
- WICKENS, D. D. (1943a) Studies of response generalization in conditioning. I. Stimulus generalization during response generalization. *J. exp. Psych.*, 33, 221-227.
- WICKENS, D. D. (1943b) Studies, etc.: II. The comparative strength of transferred and non-transferred responses. *J. exp. Psych.*, 33, 330-332.
- WICKENS, D. D., and WICKENS, C. (1940) A study of conditioning in the neonate. *J. exp. Psych.*, 25, 94-102.
- WICKENS, D. D., and WICKENS, C. D. (1942) Some factors related to pseudo-conditioning. *J. exp. Psych.*, 31, 518-526.
- WILLIAMS, S. B. (1938) Resistance to extinction as a function of the number of reinforcements. *J. exp. Psych.*, 23, 506-522.
- WOODBURY, C. B. (1943) The learning of stimulus patterns by dogs. *J. comp. Psych.*, 35, 29-40.
- ZENER, K. (1937) The significance of behavior accompanying conditioned salivary secretion for theories of the conditioned response. *Amer. J. Psych.*, 50, 384-403.

PART V

PSYCHOLOGY OF AESTHETIC EXPERIENCE

AESTHETIC EXPERIENCE IN THE LIGHT OF CURRENT PSYCHOLOGY

MAX SCHOEN

Carnegie Institute of Technology

The emphasis in psychology today, both implied and expressed, is upon a biological conception of psychological events and a view of original nature as undifferentiated mass activity. The biological outlook conceives of psychological events as processes through which an adjustment is established between organism and environment; in other words, that the psychological nature of a living body consists of the specific activities in which that body engages in order to maintain its life, while the new view of original nature holds that these activities emerge gradually by a process of differentiation out of native, non-specialized, mass activity, in contrast to the previously held view that development consisted of an integration into larger units of what were at first discrete elements of behavior and experience. The adjustive and mass conceptions of psychological phenomena fit well together, which fact may be considered as favoring their soundness, in that as acts become increasingly more differentiated out of original mass behavior they also become more and more adjustive as definite responses to specific situations.

The purpose of this paper is to present a view of aesthetic experience that fits into this picture of what human nature is and of how it develops, and to show that this view embraces every characterization of aesthetic experience to be found in the outstanding aesthetic theories of philosophy. I shall first trace the development of common experience as pictured by current psy-

chology, then indicate where the aesthetic belongs in this picture, and finally compare the result with the aesthetic theories of philosophical origin.

According to current psychology, every experience occurs originally as an organization of sensory impressions. Some experiences begin as integrations of sensory events, other as coordinations of them. Thus, a musical tone, an odor, a taste, are integrations, or blends of sensory elements, in which each element loses its identity in the whole, and can be isolated only by a direct act of attention, while a tree, a painting, a face, are coordinations, in which each part of the whole retains its identity, but all of them present a single impression of belonging-togetherness. But both integrations and coordinations are at first vague, blurred outlines, and therefore indistinct and undifferentiated from each other. This original vagueness of wholes is displaced in time by definiteness as the parts or details of the whole come more and more to the forefront. Whatever a thing is, it is that in terms of its components. These give it not only body, but uniqueness of distinctiveness. Consequently, the more that the parts of a whole are outstanding, the more distinct and definite does the whole become. A simple example of this relation between definiteness and detail may be drawn from tonal timbre, of that attribute of tonal experience by which a tone is referred to its source. A violin tone is readily distinguished from a cello tone. But the distinction can be made to disappear by eliminating certain of the partials from each tone. As the parts of several wholes recede into the background the wholes becomes indistinguishable from each other. Thus the first process in the genesis of finished experience is a differentiation between wholes as their components become increasingly clear. By differentiation chaos becomes order.

Simultaneously with the process of differentiation goes on the process of interpretation, or the process that makes experience meaningful. As experience becomes increasingly definite by differentiation it also becomes increasingly meaningful by interpretation. Life calls for action, for adjustment. But so long as a situation is vague the behavior it stimulates is uncertain, explora-

tory, and the adjustment is incomplete. As the situation becomes more definite it also becomes a clue, a sign, for a specific act, and only specific acts are effectively adjustive acts. The meaning of a situation is therefore the behavior it provokes, and definiteness of behavior is in proportion to definiteness of stimulus. Consequently, when experience has reached the stage of interpretation it is complete experience, for then the biological function of adjustment has been fulfilled. Nothing more is needed. This is the story of experience as psychology presents it. Life satisfies its needs, maintains itself, through the environment, and it learns to know that environment through the stages of differentiation and interpretation. The organism can dwell in a situation, has established an equilibrium between itself and a situation, when the situation is clearly distinguished from other situations, enabling a distinctively appropriate response to it.

Where does aesthetic experience come into this picture? Obviously, aesthetic experience can arise only from ordinary experience by doing something to it. And there are but two things that can happen to ordinary experience: adding something to it, or taking something from it. But the only thing that can be added to ordinary experience is more of itself, namely, more differentiation and interpretation, and these only make it more ordinary. Hence, aesthetic experience must arise by the elimination or abstraction from common experience of that which makes it common.

Now it cannot be the product of the process of differentiation that stamps experience as common, since without differentiation there would be no experience other than vagueness. What makes experience useful, biologically adaptive, and stamps it common, is the fruit of the process of interpretation, namely, meaning as definite response to a definite situation. For ordinary experience, therefore, the product of differentiation is but a sign for an act, a means to an end, the end being an act that satisfies a need of the organism. For ordinary experience the world of objective phenomena is but a storeroom of labels for this or that act in the service of biological existence. It is, in the language of Schopenhauer, intellect in the service of the will to

live, or, as Bergson has it, that since life is action, it "implies the acceptance only of the utilitarian side of things in order to respond to them by appropriate reactions; all other impressions must be dimmed or else reach us vague and blurred." The aesthetic experience therefore must arise by an act of imagination in which experience is rid of that which is imposed upon it by biological necessity in interpretation, so that what is left is the world of differentiation, or a world of forms. Aesthetic experience is thus form become significant as form, or intrinsic meaning displacing extrinsic meaning, in which both mind and matter become pure existence. It is the essence of experience cleansed of its attributes in the imaginative act of abstraction. It is a condition in which experience rather than the fruit of experience is the end, as Walter Pater has it, of in the words of Schopenhauer, the world become "the Will-free Subject of Knowledge . . ." The aesthetic condition is one of absorption in the object of experience, as contrasted with practical experience, in which interest is centered on the experiencing subject seeking some satisfaction through the object of experience. The practical and the aesthetic are the only two ways of experience and exclusive of each other. The one is calculating, questioning, partial, greedy, assertive, demanding; the other is contemplative, inviting, wholesome, and surrendering. Both are co-extensive with the whole realm of experience in that the one or the other attitude is possible towards any presentation. But the incompatibility between the two is final and fatal. Neither can yield the least to the other and remain its true self. In the one life is a becoming, in the other it is a being. The one seeks to live, the other lives. Where in the one thought and emotion struggle to attain, in the other both find repose in attainment. The two present life in its only two aspects: as a process of fulfilling and as a condition of fulfillment.

From this contrast between the practical and the aesthetic, the adjustive function of the aesthetic stands out in bold relief. Where the practical adjusts to the environment, making living possible, the aesthetic adjusts to the practical, making life endurable. Thus the aesthetic may be said to have a biological

function, since there is ample evidence that there is in human life a craving for respite from life as "an ever becoming and a never being." And it is significant that the protests against the demands of the practical come from persons most richly endowed with the will to live, namely, the outstanding creative minds of the ages. It seems that those most fully alive, and therefore most eager for life, also find burdensome the incessant demand of life for more and more of the same, and desire release, a vacation, from themselves, in order to come back refreshed to themselves. What one is most eager for also oppresses him to such a degree as to create a counter desire for release from it. Death seems to have terrors only for those who have little of life to lose. The will to live so oppressed Schopenhauer that its negation by death, or at least by the immortality of the Platonic Idea, became an obsession with him. Hence, his profound conception of the aesthetic and his glorification of genius as the objectification—which amounts to the annihilation—of the will. Had he not been excessively driven by life he could not have longed for the peace of death. Keats longs to forget

The weariness, the fever, and the fret
Here, where men sit and hear each other groan;
Where palsy shakes a few, sad, last grey hairs
Where youth grows pale, and spectre-thin, and dies,
Where but to think is to be full of sorrow
And leaden-eyed despairs;
Where beauty cannot keep her lustrous eyes,
Or new Love pine at them beyond tomorrow.

Wordsworth deplors our laying waste our powers with getting and spending, giving our hearts away, a sordid boon, our being out of tune to the pure experience of

This sea that bares her bosom to the moon;
The winds that will be howling at all hours,

and proclaims that he'd rather be

A pagan suckled in a creed outworn;
So might I, standing on this pleasant lea,
Have glimpses that would make me less forlorn,
Have sight of Proteus rising from the sea
Or hear old Triton blow his wreathed horn.

Even Nietzsche's Superman is but a gesture of defiance or challenge at life on the part of a weary soul, and who knows but that skepticism, cynicism, pessimism, optimism are each of them but escape mechanisms, consolations for living. There are no skeptics, cynics, pessimists, or optimists among human beings who are no more than vegetative systems. The aesthetic seems to be the one wholesome, good-natured antidote of the practical, for it is not a denying or a rejecting of it, but a purification of it and a resting from it. The purgation of an object of its impurities is also a catharsis of the subject, for as imagination divests experience of its extrinsicities it also rids life of its strivings. The aesthetic arises as a protest against the demand of the practical upon life to become, declaring its inherent right to be. Being and becoming are supplementary, the one calling upon the other as its corrective, for becoming without being exhausts life and being without becoming annihilates it. The one keeps life moving, keeps it alive, the other enables life to savor of itself, to taste of the joy of living.

At the basis of the aesthetic response there must function a high degree of susceptibility to the sensory stuff of experience, for when the intrinsic aspect of experience is not sufficiently vivid to be meaningful in itself meaning can come only from the extrinsic. It is like a person, who, if he has not enough business of his own to mind will mind other peoples' business, if he is to have anything to mind. If genius is then a condition of objectification of the will, it is also true that the sense of genius, as has been claimed, "are not narrow paths, but broad highways whereon march armies of impressions, thronging to the citadel of his mind." And there is some experimental data pointing to the conclusion that aesthetic response to music is a function of the degree of sensitivity to the sensory material of music, namely, that persons who cannot find meaning in music as such are driven to seek its meaning in extra-auditory imagery. Walter Pater here seems to be in the right when he maintains that it is the sensuous element of art that is essentially artistic, from which follows his thesis that music, the most purely formal of the arts, is also the measure of all the arts. This high sensitivity to the intrinsic in

experience may also be the basis for the protest against the demands of the practical for preoccupation with the extrinsic, for the extrinsic is an imposter, an intruder, where the intrinsic holds sway. Speaking in psychological terms, this amounts to saying that degree of sensitivity to the details of the organized whole determines the degree of the significance of the whole as such, and that when the sensitivity is not sufficient to make the whole definite enough to engender meaning, this has to be sought, if it is to be found at all, outside the directly presented. When the directly presented is vague it can become definite only in terms of something other than itself.

The view of the aesthetic as the realm of pure experience, or of form become significant as such, seems to be corroborated by the common implication of the words beautiful and artistic, as well as by the theories of beauty.

The term "beautiful" is frequently used outside the realm of art to indicate a unique value attached to common-place events. Thus we speak of a beautiful idea, a beautiful act, a beautiful movement, a beautiful friendship, and so on. Now what does beautiful signify in those connections? What is it that is beautiful in the idea, act, movement, or friendship? What is the difference between a good idea and a beautiful idea? The goodness of an idea comes from its implications. It is good because of its usefulness. The goodness is a derived value, extrinsic to the idea itself. The same holds for a true idea. It is true always in terms other than itself. The idea is beautiful, then, because it is prized for what it is. It has intrinsic value, and as intrinsic it is a form. Similarly, a good act is good in terms of some function it has performed, while the beauty is in the act itself, and the act itself is a form.

The terms art and artistic also refer to form. A product is never judged to be art just because it is a product of a particular class of objects. If a painting is art it is not such because it is a painting of a field, a tree, a barn, or a person. Nor is a drama or novel an art product just because it deals with a particular human situation. What is left, then, is, again, form. If prose is judged to be artistic it is the style, the form of presentation, that

is artistic, not the subject-matter of the prose. The ideas may be false, absurd, yet the writing can be artistic. When something is artistically done, it is not *what* is done that is artistic, but *how* it is done. Thus, the aesthetic, as experience, is pure experience, and as activity, is pure activity. It is the aristocrat of experiences, excluding anything and everything that is not of its essence. In the aesthetic the realms of phenomena will be prized on their own merits, for whatever they are and as they are, or they vanish into oblivion.

The definition of the aesthetic as an abstraction of the practical is also supported by the theories of beauty, since every one of the theories states in varied terminology that beauty is pure experience, or experience of form versus experience of content. Those who find a contradiction among aesthetic theories have simply not taken the trouble to grasp the gist of each theory, and assume that a difference in terminology also represents a different conception of the nature of that to which the terminology refers. The fact of the matter is that each theory stresses, or perhaps overstates, some one characteristic of the complex experience and calls that the whole experience. Thus some theories concern themselves with the nature of the experience itself, others with what the experience is expressive of, and some with what the experience does to the experient. To the first class belong the theories of *intrinsicity*, *disinterestedness*, and *objectification*; to the second the theories of *significant form* and *intuition*; and to the third those of *psychical distance*, *aesthetic repose*, and *catharsis*. It can be shown not only that the theories in each group are aspects of each other, in that each includes the others, but that all the theories imply the aesthetic as the negation of the practical.

Disinterestedness, intrinsicity, and objectification belong together, in that a disinterested attitude gives rise to an intrinsic experience, and in an intrinsic experience the subjective is objectified. However, the disinterestedness of the aesthetic must be distinguished from that of science or philosophy. In science and philosophy disinterestedness is deliberate because of the desire to reach a truth supported by fact. It is a search for objectivity,

for an unprejudiced conclusion. In the aesthetic the disinterestedness arises from an interest that attains an absorption in what is directly and immediately perceived, and in such disinterestedness there is objectification, since subject dwells in object. Santayana puts the cart before the horse when he defines beauty as pleasure objectified. It is not the objectified pleasure that accounts for the presence of beauty; it is rather the presence of beauty that makes for the objectification of the pleasure. In other words, in beauty the person dwells in the object, so that the feeling of pleasure is also experienced as dwelling in the object. But this a mere matter of detail, the important point being that any one of the three characteristics of beauty includes the other two.

The theories of significant form and intuition come to the same thing, in that both call attention to what it is that is experienced in beauty. Bell defines significant form as the common quality in all visual art—there being no reason for his limiting it to vision excepting that his book is limited to painting—and this common quality lies in lines and colours combined in a particular way, certain forms and relations of forms that stir the aesthetic emotions. Croce contrasts intuition with intellect by calling intuition the process by which we obtain knowledge of individual things—or differentiation—while intellect gives knowledge of relation between things—or interpretation. His examples of intuitive knowledge are an impression of a moonlight scene by a painter, a musical theme, the words of a singing lyric, all of which are "intuitive facts without a shadow of intellectual relation", and as such they are forms.

Of the third group of theories that of catharsis is the oldest and most widely known. Aristotle states in the *Poetics* that it is the service of tragedy to effect a proper catharsis or purgation of the emotions of pity and fear, and attributes such an effect also to music in the *Politics*. Now whereas Aristotle nowhere explains directly the nature of this purgation, we can infer what he had in mind from his definition of fear in the *Rhetoric* as "a species of pain or disturbance issuing from an impression of impending evil which is destructive or painful in its nature." It is clear, from this definition, that a catharsis of fear must consist

of the elimination from it of the "impression of impending evil," in which case the fear becomes, in the words of Butcher, "an almost impersonal emotion, attaching itself not to this or that particular incident, as to the general course of action which is for us an image of human destiny." In other words, the emotion is stripped in imagination of its biological implications and becomes intrinsic experience. In such case, the restlessness, the drive to action, that is the predominant trait of every-day emotion, also disappears, resulting in a state of repose in tension. And this is Puffer's definition of beauty as aesthetic repose. There is, in beauty a state of aesthetic repose through catharsis. Bullough's theory of psychical distance states the same thing in different language. If a person is psychically distanced from himself when standing on a hillside with lightning playing about him he is at repose because he is engrossed in the phenomena rather than in the thought of the danger to himself. Psychical distance is but another name for catharsis, only it explains how the catharsis is effected. It is this condition of aesthetic repose resulting from catharsis by psychical distance that leads to the characterization of aesthetic experience in such mystical terms as the perfect moment, a moment of eternity, the annihilation of self, experience that is timeless and spaceless, contact with ultimate reality, and several other similar descriptions. Aesthetic repose, being a state of total equilibrium of forces, is a perfect moment because it is a moment of fullness and completeness, free of straining and striving, and therefore also a moment that is eternal, since consciousness of time and space are phenomena of the will to live, and it is the will to live that generates self-consciousness. A will-less, self-less state, which is a state of perfection and eternity, also presents ultimate reality, because beyond that experience cannot reach out.

Now, when we put the three classes of aesthetic theory together, what do we find? We find that disinterested-intrinsic-objectified experience is experience of form become meaningful as form, and in such experience there is repose, a catharsis of striving, which makes for a perfect moment, a moment that marks eternity, a moment that is therefore timeless and spaceless and

therefore also self-less, marking a moment of finality or of ultimate reality.

To summarize: every characterization of aesthetic experience in aesthetic theory arises from the experience of form, which is the result of divesting experience of its every-day garb of the practical, or, in psychological terminology, the ridding of experience of the accretions from the process of interpretation, leaving for contemplation the fruit of the process of differentiation, which is the world of pure being, the world of forms. Psychologically, then, aesthetic experience is primitive experience, or experience of the organized whole become significant as such in the fullness of its detail.

THE EXPRESSION OF MEANINGS AND EMOTIONS IN MUSIC ¹⁷

MELVIN GILLISON RIGG

Oklahoma Agricultural and Mechanical College

I

In formulating the rules for his ideal republic, Plato forbade the use of the Lydian and Ionian modes, since he believed them to be suggestive of sorrow and dissoluteness. On the other hand, the Dorian and the Phrygian modes were approved because they were thought to stand for courage and for temperance, respectively. Plato likewise sensed a moral difference in rhythms; a "good" rhythm typified the courageous and the harmonious, while a "bad" rhythm signified meanness, insolence, or fury.¹

Plato is not alone in this assumption that music can suggest that which is beyond its immediate realm. It is almost universally assumed that a composition can express emotions, that it can be joyful or sorrowful, exciting or restful, that it can depict triumph or yearning. Many persons would go farther and claim that music can present definite meanings, that it can portray the spring, the early morning, a brook, a spinning wheel; or the composition may recount the various escapades of an outlaw, his apprehension, condemnation, and execution. Program commentators and music critics make their living by retailing such ideas, and so great is the trade demand that if the composer leaves no interpretation of his production, it is usually not long before one is invented.

¹ *Republic*, Book iii.

Perhaps this desire to read non-musical significance into music is but one instance of a more widespread tendency of the fine arts to encroach upon each other. Such terms as tone in a picture, and color applied to music, while understandable as synonyms for intensity and timbre, seem to be evidence of this overlapping. As against the tendency, Lessing has stated in his *Laokoön* the conviction that poetry should not try to paint a picture nor painting to depict action. Should music become a party to a similar non-aggression pact?

But any effort to resist aggression must involve a delineation of exact boundaries. Just how far does the sphere of music properly go? To what extent is it possible for music to express meanings and emotions?

It is first necessary to note that a composition may occasionally imitate natural sounds. In his *Siegfried*, Wagner produces effects similar to the songs of birds; Strauss likewise, in *Don Quixote*, simulates the bleating of sheep by means of muted brass. Perhaps the most conspicuous example of all is Rimsky-Korsakoff's *Flight of the Bumblebee*, a clever bit of orchestration which is useful as an encore number to afford some relief to non-musical members of the audience.

Next may be considered the significance which comes as the result of association. Music enters readily into associative bonds. A piece which the writer practiced as a boy always suggests Egypt to him, since he was reading a book at the time dealing with that country, although the composition is, as a matter of fact, Polish, and has nothing Egyptian about it. Such a connection is an individual affair; this selection would not mean Egypt to anyone else. But in the case of other works, associations may be widespread in the popular mind, and if the traditional meaning is known, it is easy to feel the appropriateness of the musical setting. Mendelssohn's *Wedding March* arouses the thought of orange blossoms, and the *Star Spangled Banner* that of patriotism. We have never heard the music of the latter in its original form as a drinking song. Also we fancy that the *Moonlight Sonata* really depicts moonlight and that there is something swanlike in the familiar piece by Saint-Saëns.

Even more troublesome are the conventional meanings which have become attached to certain detailed features of music. Boats move traditionally in 6/8 time whether they be gondolas, sailing vessels, or canal boats; the *Barcarolle* from the *Tales of Hoffmann* is a familiar example. Horses are suggested by phrases in triplet rhythm, such as are found in the *Erlkönig*.² Even in Mendelssohn's *Elijah*, the horses which come down from above to carry the prophet to heaven, and are neither trotting nor galloping but *flying*, are revealed to us by these conventional triplets.³ Since in these cases the association is not with the entire opus but with a detail only, this same characteristic occurring in a work entirely new to the listener may suggest its accustomed meaning, and may thus establish a conviction that the significance is intrinsic.

II

The composer is usually accorded a privileged position in the interpretation of his own works. If he announces that his symphony represents the restless striving of the human spirit against the decrees of Fate, then for any other musician to fail to find this meaning becomes a confession of musical insensitivity. It may be suspected that we have here a case analogous to that of the Emperor's clothes, no one daring to reveal that he does not see a non-existent significance.

The value of an experimental approach to this problem seems obvious. How accurately are the intended meanings of composers actually conveyed to listeners by the music alone? The present writer decided to ascertain what would be the results of such an investigation in his own classes.⁴

² In the symphonic poem *Phaeton* by Saint-Saëns, passages in triplet rhythm alternate with those containing a closely similar figure of two sixteenth notes and an eighth.

³ I am indebted to my former teacher, Professor H. A. Clarke, for pointing out to me both of these conventions.

⁴ Melvin G. Rigg, "An Experiment to Determine How Accurately College Students Can Interpret the Intended Meanings of Musical Compositions," *Journal of Experimental Psychology*, v. 21 (1937), pp. 223-29.

Eighteen Victrola records⁵ were accordingly selected and played to seventy-four college students. The records were heard at two different sittings to avoid fatigue. Each auditor was provided with the following outline for his guidance:

- I. Sorrowful, serious, religious, etc.
 - A. Death
 1. Death scene
 2. Funeral march
 3. Elegy
 - B. Sorrow
 4. Sorrow in general
 5. Farewell
 - C. Religion
 6. Good Friday music
 7. Prayer
- II. Joyful (love, activity, nature, etc.)
 - D. Love
 8. Love song
 9. Serenade
 - E. Activity
 10. Spinning song
 11. Cradle song
 12. Dance
 - F. Nature
 13. Morning
 14. Moonlight⁶

The auditors were asked to judge first whether the music was predominantly sad-serious or joyful, and they entered either I or II on the blanks provided. Then they were to try to make a more accurate judgment and tell whether the piece portrayed

⁵ These records were: *Siegfried's Funeral March* (from *Götterdämmerung*), Wagner; *Butterfly's Death Scene* (from *Madame Butterfly*), Puccini; *Swedish Cradle Song* (Victrola record No. 3004-B), folk song; *Elegy*, Massenet; *At Dawn* (from *William Tell Overture*), Rossini; *Adagio Lamentoso* (from *Symphonie Pathétique*), Tschaikowsky; *King's Prayer* (from *Lohengrin*), Wagner; *Garden Music* (from *Faust*), Gounod; *Turiddu's Farewell* (from *Cavalleria Rusticana*), Mascagni; *Fatal Stone* (from *Aida*), Verdi; *Moonlight*, Debussy; *Farewell* (from *La Bohème*), Puccini; *Elizabeth's Prayer* (from *Tannhäuser*), Wagner; *Good Friday Music* (from *Parsifal*), Wagner; *Death of Boris* (from *Boris Godounow*), Moussorgsky; *Omphale's Spinning Wheel*, Saint-Saëns; *Serenade* (from *Don Giovanni*), Mozart; *Death and Transfiguration*, Richard Strauss.

⁶ This outline was made to accommodate the actual selections that were used; it is not designed as a universal system of classification for music.

death, sorrow, religion, love, activity, or nature. This they did by entering on their reports one of the capital letters from A to F. After that a still finer discrimination was to be attempted and the students were to indicate whether the composition represented a death scene, a funeral march, an elegy, etc., by using the Arabic numerals from 1 to 14.

In any such experiment it is of course necessary that the titles or traditional meanings of the selections be unknown to the persons serving as judges. The observers were consequently asked for each item if they had heard the composition before, and if so, what was the name of it. Most of the music was not recognized, and in only one per cent of the cases could the name be given correctly. When a student knew the title, his reply to this item was not counted in the data. Occasionally a word or two of the songs would be discriminated, but most of the vocal numbers were in foreign languages, and the one which was in English was sung by a foreigner, so this source of error was not extensive. The student was asked, when he heard a significant word, to record the fact, and his response to this number was likewise left out of the tabulation.

The results of the experiment were that 73 per cent of the time the auditors could tell whether the music was intended to be sad or joyful; 41 per cent of the time could further classify the pieces into the categories death, sorrow, religion, love, activity, and nature. Only 25 per cent of the time could they make the still finer classification as indicated on the outline. The percentages are in each case better than chance, but in only the first discrimination is there a majority of responses for the right category.

There was naturally a great variability among the different compositions. In the third or finest discrimination the *Swedish Cradle Song* was correctly classified by 64 per cent of the group, the *Garden Music* from *Faust* by 56 per cent (perhaps because it is a duet for soprano and tenor), *Omphale's Spinning Wheel* by 50 per cent (probably because of the rhythm), and the *Serenade* from *Don Giovanni* by 38 per cent (possibly because of the

characteristic accompaniment). Strauss' *Death and Transfiguration* was correctly placed by 36 per cent and called a funeral march by another 34 per cent.

On the other hand, Massenet's *Elegy* was considered to be a love song or a serenade. If grief was intended to be represented, the attempt was not in this case successful. *At Dawn*, a portion of the *William Tell Overture*, was by more than half of the judges regarded as expressing some sort of sorrow. Twenty per sons (28 per cent) identified the *King's Prayer* from *Lohengrin* as a prayer, but nineteen others (26 per cent) thought it was a serenade. *The Farewell* from *La Bohème* was put down by almost half of the auditors as a love song; the sorrow of the selection was not recognized. In the case of the *Good Friday Music* from *Parsifal*, 18 per cent of the listeners thought it a serenade; only 15 per cent said that it was religious and only 7 per cent classified it as Good Friday music. This latter figure represents merely the chance distribution.

In spite of the instances of correct placement, the percentage of success for the experiment as a whole seems low, especially in view of the fact that the students were supplied with outlines which gave them a clear idea of what was coming. It might be too much to expect anybody hearing music at random suddenly to realize that a selection would be appropriate, let us say, for Good Friday. But when the category "Good Friday music" is one of the fourteen terms of an outline, the small number of correct responses appears as a more serious failure.

The question as to the influence of training upon any sort of musical judgment is a constant one, and in this instance an answer was sought by dividing the listeners into three groups on the basis of their education in music. The high and low groups were then compared, and it appeared that those with the most training could make correct placements (in accordance with the composer's intention) only slightly oftener than those with the least training. In the first discrimination, for instance, the highly trained group had a percentage of 75 against a percentage of 71 for the relatively untrained group. This difference is too small

to be reliable,⁷ and it seems doubtful if training makes any practical difference in the ability to judge the emotional significance of a selection.⁸

The negative results of the experiment are surprising to many persons. The objection has been made that the observers must have been insensitive to music. It is true that they were not conservatory students, but they were enrolled in a college which is superior to the average for American institutions of learning; moreover, some of them had studied music for many years. It is consequently hard to escape the conclusion that the intentions of composers usually do not "get over" in any specific way to the cultured strata of our population.

III

Several of those who communicated with the writer with reference to the experiment just described have maintained the position that at least the more general moods may inhere in compositions. Perhaps the *Moonlight Sonata* does not necessarily suggest moonlight, but it is more appropriate for moonlight than such a piece as the *Stars and Stripes Forever*. After learning what the intention of the composer was, it is often possible to realize that the mood is suitable, although the meaning could not have been conveyed by the music alone.⁹

Undoubtedly the problem is not an all-or-none proposition; it is a question of how much and in what directions, and the next step would seem to be one of analysis. Some of the difficulty of judging a work occurs from the fact that it may not be in the same mood throughout. Strauss' *Death and Transfiguration*, for in-

⁷ The ratio of the difference to its own standard error is .33, which means that if the experiment were repeated 100 times we could expect the slightly trained group to excel the highly trained group in 37 instances.

⁸ This result is in agreement with a similar conclusion announced by Hevner to the effect that trained musicians are able to judge the emotional significance of the major and minor modes only slightly better than untrained observers. Kate Hevner, "The Affective Character of the Major and Minor Modes in Music," *American Journal of Psychology*, v. 47 (1935), pp. 103-18.

⁹ This is what the writer understands to be the view of Mr. Deems Taylor, who discussed this experiment on his New York Philharmonic broadcasts for February 20 and February 27, 1938.

stance, contains a theme in marked contrast to the rest of the composition. But even if attention is centered on a single phrase, just what features of this phrase account for its emotional significance?

An elaborate attempt to analyze music with the purpose of identifying such characteristics has been made by Erich Sorantin.¹⁰ After a study of numerous works in which the intentions of the composer were evident from text or title or were otherwise known, he has selected certain features as being symptomatic of five different emotions, as follows:

<i>Lamentation</i>	<i>Joy</i>
slow tempo	accelerated tempo
descending minor seconds in melody	ascending fourths in melody
minor mode	major mode
legato phrasing	staccato notes
trochaic rhythm	iambic or anapaestic rhythm
dissonance	simple harmony
low register	forte dynamics
<i>Sorrowful Longing</i>	<i>Hopeful Longing</i>
melodic intervals and chords of seventh or ninth combined with features of lamentation	melodic intervals and chords of seventh or ninth combined with features of joy
<i>Love</i>	
dolciata S curve melody ¹¹	
thirds and sixths in treble	
major mode	
moderate tempo	
legato phrasing	
melodic intervals and chords of seventh or ninth	

Sorantin has presented no experimental proof that persons who hear the above musical patterns actually sense these emotional qualities. The present writer, in an attempt to supply such evidence, has performed five additional experiments, in all of which short musical phrases were played to auditors, who recorded their impressions of each passage. The number of judges varied in the experiments from 84 to 105. These were for the

¹⁰ Erich Sorantin, *The Problem of Musical Expression*, Nashville, Marshall and Bruce, 1932.

¹¹ A dolciata S curve melody is one having an upward and downward oscillation.

most part college students, who naturally possessed varying degrees of musical training. The five experiments were concerned with 129 different phrases and involved 15,739 separate judgments.

The first experiment¹² was concerned chiefly with examples from the literature of music selected by Sorantin to illustrate his theories. However, some of the phrases were made to order by the present writer by a process of putting into them everything that was called for by one of the patterns and of excluding everything else. This experiment seemed to show that Sorantin's theories for lamentation and joy are well substantiated, but that the theories for sorrowful longing, hopeful longing, and love are less certain. Strangely enough, the phrases written for the experiment confirmed the theories better than did the passages selected from the classics. This outcome does not mean that these original phrases are better music, but evidently results from their custom-built method of composition.

The chief limitation of the experiment was that it merely tested Sorantin's theory for each pattern as a whole. If a phrase combined all of the lamentation characteristics, descending minor seconds, minor mode, dissonance, etc., the data showed that it did actually suggest lamentation. But there was no evidence as to which of these features were the most important; indeed some of them might have had no effect at all, or might possibly have exerted an influence which weakened the total emotional significance of the phrase. The next step in the research was to determine the influence of the several characteristics, both individually and in various partial combinations.

The four subsequent experiments¹³ all involved variations

¹² Melvin G. Rigg, "Musical Expression: An Investigation of the Theories of Erich Sorantin," *Journal of Experimental Psychology*, v. 21 (1937), pp. 442-55.

¹³ Experiments II and III have been published jointly. M. G. Rigg, "What Features of a Musical Phrase Have Emotional Suggestiveness?", *Publications of the Social Science Research Council of the Oklahoma A & M. College*, No. 1, 1939. Experiment IV was published as, "Speed as a Determiner of Musical Mood," *Journal of Experimental Psychology*, v. 27 (1940), pp. 566-71. Experiment V appeared as, "The Effect of Register and Tonality upon Musical Mood," *Journal of Musicology*, v. 2 (1940), pp. 49-61.

of the same five phrases, which are identical with (or closely similar to) original phrases used in the first experiment. The second in the series of investigations is called the Single Variation Experiment, since each additional phrase was modified from its original in one feature only. For instance, the original phrase designated as A-1 embodies the seven lamentation characteristics previously enumerated. Each additional phrase embodies a modification of one of these seven features, the other six remaining the same. The third investigation is called the Cumulative Experiment. The phrase A-11, for example, differs from A-1 in two features (it has no dissonance and possesses ascending seconds). The phrase A-12 differs from A-1 in these two respects and also in one other characteristic, and each subsequent phrase varies in one additional feature until all seven of the characteristics of A-1 have been contradicted. The fourth research is the Tempo Experiment, each of the five original phrases being played at six different metronome speeds. Experiment V was concerned with register and tonality; each of the phrases being shifted (1) either up or down an octave, (2) either up a fifth or down a fourth to the key of the dominant, (3) up one half-step, (4) down one full step.

In Experiments II to V the auditors were assisted in making their judgments by the same check list. This is a matter of importance, since to a certain extent replies are determined by the categories the list contains. From Experiment I it appeared that the most valid discrimination is between the happy and the sad. Consequently the students were first asked whether the phrase was primarily sad-serious, designated as X, or pleasant-happy, designated as Y. After this judgment the observers were to attempt a finer discrimination by classifying the phrase under sub-headings. The complete outline is as follows:

X Serious-sad

1. Solemnity
2. Sorrowful longing
3. Melancholy
4. Lamentation
5. Agitation

Y Pleasant-happy

6. Hopeful longing
7. Love
8. Revery
9. Gaiety
10. Joy
11. Triumph

These terms were chosen with two purposes in mind: (1) to include all the categories used by Sorantin so that his theories might have ample opportunity to prove themselves; (2) to include certain other terms suggested by the participants of a preliminary tryout. It was explained to the auditors that for these experiments melancholy meant something less intensely sad than lamentation, while gaiety was less intense than joy.

The five experiments showed in their results a consistency which was beyond expectation. Some of the conclusions are summarized:

The most important feature, in so far as emotional suggestiveness is concerned, appears to be tempo. Fast tempo weakens the effect of lamentation; a phrase which when played at $J = 60$ meant lamentation to 55 persons, suggested this emotion to only 12 persons when played at $J = 108$. Slow tempo destroys the joy-effect; a different phrase, suggesting joy to 49 judges when played at $J = 160$, attracted only 8 responses for this category when played at $J = 72$. So important did this finding appear in Experiments II and III that Experiment IV was devoted to tempo alone. The five original phrases, two of which had been established as sad and three as happy by the previous investigations, were played, each at six different metronome speeds. The general rule was found to hold that an increase of speed meant progress along the dimension from sadness to happiness. Sometimes the evidence was in the sub-headings, changes in the number of choices for lamentation or joy, but for the most part it was clearly registered in the shifts of the X and Y responses.¹⁴ The different features are of course interdependent and the emotional quality of the phrase is the result of the total pattern. Fast tempo may weaken the lamentation effect, but it is not enough by itself always to make a phrase happy. In the case previously cited where the lamentation replies went down from 55 to 12, nevertheless the responses remained overwhelmingly on the X side of the outline.

¹⁴ The only partial exception to this result was that for the phrase C-1, somewhat sorrowful in character, there was no significant change between two adjacent slow speeds ($J = 60$ and $J = 80$).

Staccato notes also furnish a good example of this interdependence of factors. Introduced into a phrase containing characteristics of sorrow, the staccato notes suggest agitation; embodied in another phrase devoid of sorrow features, the staccato notes make it happier and tend to suggest gaiety.

The major mode has an influence away from lamentation and in the direction of the Y side of the outline, but is not enough by itself always to make the phrase happy. On the other hand the minor mode has a tendency away from joy and in the direction of the X side of the outline, but is not enough by itself always to make a passage sad. Mode is on the whole not so important as tempo.

Changes in register were found to be very important, a shift up producing results in the direction of joy, while a shift down made the phrase sadder. If the difference was as much as an octave, this rule held without exception. Changes of approximately half an octave, transpositions up a fifth or down a fourth to the key of the dominant, produced results somewhat less consistent; out of ten instances the rule held in eight cases and the variations in response were usually slight. The effect of joy was also enhanced by a trill at high register, but in this case the speed of the trill was no doubt an important additional influence.

However, for many details of the Sorantin theory no evidence could be found. One of the features of lamentation was supposed to consist of *descending* minor seconds, but it apparently made little difference whether these seconds went up or down. Joy is supposedly suggested by *ascending* fourths, but descending fourths seemed to produce about the same results. Longing and love were both associated in the theory with intervals and chords of the seventh, but when these were omitted, the results were not materially changed. The thirds and sixths in the treble and the *dolciata* S curve melody, both features of love according to the theory, likewise seemed to have little significance.

With regard to another matter, not a part of the Sorantin theory, the evidence was also negative. Musicians have attached enormous importance to the matter of tonality. The different keys have been assigned distinctive characters, so that the selection

of a key for a new composition is regarded as requiring a fine perception of fitness. In Experiment V, by transposing the phrases down one step or up one half-step, large changes in tonality were produced, although the changes in register were slight. There were ten such shifts in the fifth experiment, and in eight instances no changes of any consequence in the distribution of replies could be observed.¹⁵ Therefore, this investigation lends no support to the view that different keys possess peculiar qualities.

IV

But to return to the positive results: there are certain characteristics of music which really seem to suggest sadness, joy, agitation, and perhaps triumph. The most important features appear to be tempo, staccato notes, mode, and register.¹⁶ How may these results be interpreted?

In the first place, to what does the emotional quality belong? It is a common experience to realize that the mood of the music is different from one's own. "That is happy music," you may say, "but I am not happy, and it irritates me." Is the emotional quality then in the music? There would be many who would hesitate to accept the metaphysical implications of this position. "Joy and sorrow, agitation, and triumph, these," they would say, "are conscious states and can inhere only in a sentient being." And such persons could probably construct for themselves a satisfactory theory based upon the principles of association and empathy. Through long experience, they would claim, we have connected some features of music with joy and others with sorrow, and in certain ways we reinforce these connections by putting ourselves into the music.

¹⁵ In the other two instances the results were as follows: One change from C major to B flat major produced a 17 per cent loss for joy and a 12 per cent gain for gaiety. The other change from F minor to E flat minor brought about a 19 per cent shift from melancholy to sorrowful longing. None of these shifts is *statistically* significant in the sense of having a critical ratio of 3, although in one case the ratio is 2.9. In view of the inconsequential results from the other eight instances, it is questionable whether any conclusion can be drawn from these two cases.

¹⁶ These lists are not of course exhaustive. They are merely the results of the present series of experiments.

Perhaps the joy-picture is illustrated by the little child bubbling over with exhilaration of living and apparently possessing boundless energy. He jumps and skips with the eager spontaneity of all young animals. We have thus come to think of rapid tempo as one of the features in the pattern for happiness. Our connection of high register with joy may be similarly explained, since laughter and shouts of merriment make use of the higher degrees of pitch.¹⁷ The sorrow pattern is on the other hand that of restraint, with actions slow or forced; we may visualize an old man sitting dejectedly and occasionally uttering low moans of despair. And since it is desirable to get away from the conventional and to approach the natural, can not additional confirmation be secured from the actions of animals? Sigmund Spaeth has told the writer that hounds start to bark in a higher pitch when they have picked up the scent of the fox. The happy dog also frisks around at a lively rate, barking excitedly, while the dejected dog slinks away, tail between legs, and his inquiring whine is in a lower register. A similar contrast may be observed between a patient in the manic phase of manic-depressive insanity, who jigs and sings, and the same patient in a later depressed phase, sitting in a drooped position and moaning.¹⁸

It should be remembered that the effect of any feature is varied by the influence of other features which may be present. There is no implication that all emotions involving rapid rate or high pitch are pleasant in character. The writer found that a sad phrase played fast, while it became less sorrowful, tended to suggest agitation. Perhaps hysterical grief is to be viewed as sorrow plus agitation. Recent research has also indicated that fear and anger as expressed by actors in speech are both characterized by high pitches.¹⁹ Since these categories were included only in the check list used in Experiment I, no evidence of any

¹⁷ There may also be another explanation in the fact that the higher pitches involve more rapid vibration rates. Thus register may ultimately be reducible to speed.

¹⁸ Low, rumbling thunder also seems sad to certain persons, perhaps because it has been associated with gloomy days.

¹⁹ Grant Fairbanks and Wilbert Pronovost, "Vocal Pitch during Simulated Emotion," *Science*, October 21, 1938, v. 88, No. 2286, pp. 382-83.

value with regard to these points has been collected by the present writer.

Staccato notes introduced into a sad passage may well remind us of the quick, jerking movements of agitation; in a happier setting the staccato notes may suggest laughter or dancing.

The features of speed, pitch, and staccato notes also undoubtedly depend for their effects upon empathy as well as association. Some persons will tap out a lively tune, humming the melody. Many others will feel the impulse to move at a rapid pace, and to hum subvocally. Perhaps the reason why happy music seems so irritating in moments of depression is simply that we feel impelled to make these joyful movements, which are in conflict with our prevailing mood.

But why does the major mode suggest joy and the minor mode sorrow? The major triad is a combination of a major third and a minor third, with the major third below. The minor triad is the same combination with the major third above. Or if only the major and minor *thirds* are considered, instead of the complete triads, why do two notes whose frequencies have the ratio 4:5 suggest joy, while two other notes with the ratio 5:6 suggest sorrow? It is true that the major third is slightly more consonant than the minor third, and slightly more agreeable, as determined by laboratory experimentation, but the differences are hardly great enough to account for the divergence in emotional effect.

Some persons hear the howling winter wind in a minor key. It is perhaps actually nearer to the chromatic scale than to the minor; however, most persons would say that the chromatic scale is closer to the minor than to the major. It has also been claimed that wolves howl in minor, and so for both these reasons the minor mode may have been connected with loneliness and the unpleasant.

It has, however, been suggested that our interpretation of the minor mode is a mere convention. Heinlein²⁰ determined that only 7 per cent of pieces for beginners are in minor; consequently these are set off in the child's mind as something abnormal. Most of these, moreover, were found to have descriptive

titles suggesting the weird or the gloomy. An explanation for this convention may be that the somber qualities of the words sung to the old ecclesiastical modes have left their stamp upon the minor keys, which to a certain extent resemble them.

However these explanations may be regarded, it appears from the experimental evidence that music can portray at least the emotions of joy, sorrow, agitation, and perhaps triumph. No doubt additional evidence will reveal other similar emotions which belong with these four. But the attempt to go beyond this general area places the matter on extremely uncertain ground. Music, it is here contended, can portray general moods, but not definite ideas. The writer would be inclined to agree with an extension of Lessing's position, and hold that music should try neither to paint a scene nor to tell a story. A composition may be lacking in vigor and convey a certain indistinctness, but does it represent moonlight? Another selection may have something in the nature of a "sweeping dignity" perhaps, but does it suggest a swan moving down a river? As a matter of fact, the mood of a piece may be appropriate to a great many different situations; it may be impossible to tell whether the intention is to represent the rustle of spring, the maiden's dream of love, or a state of religious ecstasy.

This viewpoint does not condemn the employment of music as an accompaniment or setting, and used in this way it may reinforce the various emotions presented. A happy poem should have a happy musical setting; for a tense moment in the opera the music should be agitated or "stormy." But the ideas of the poem or the story of the opera, we should obtain, not from the music, but from the words.

Is it not true that persons for whom a wealth of non-musical interpretation seems essential are those who find it difficult to understand music *as* music, and are thus impelled to seek beyond its confines that significance which within it they can not find?

²⁰ C. P. Heinlein, "The Affective Character of the Major and Minor Modes in Music," *Journal of Comparative Psychology*, v. 8 (1928), pp. 101-42.

THE AFFECTIVE CHARACTER OF MUSIC ¹⁸

CHRISTIAN PAUL HEINLEIN

Florida State College for Women

In recent years, both theoretical and experimental psychologists in various parts of the country have manifested a growing interest in the affective character of human response to certain modes of musical expression. Numerous historical, analytical and laboratory investigations have been conducted for the primary purpose of demonstrating the degree of consistency and universality which characterizes the relationships between specific affective states of experience and specific tonal patterns. Doctors Max Schoen, Kate Hevner, Carroll Pratt, Otto Ortmann, Melvin Rigg, Paul Farnsworth, Ether Gatewood, Margaret Washburn, Carl Seashore, Christian Ruckmick, J. Beebe-Center, Charles Diserens, and James Mursell are among the foremost research investigators in this country who have contributed prominently in recent years to a better understanding of the nature of human affectivity in relation to various kinds of sound phenomena. It is not my purpose at this time to review in detail the separate research findings of these investigators, nor is it my purpose to evaluate critically any differences in interpretation which may have arisen through the application of different methodologies and laboratory techniques.

The chief purpose of this paper is to consider the limits within which selected tonal patterns, either static or dynamic in character, are found to be endowed with potentialities to arouse in human experience certain discriminable states of feeling and emotion. A secondary but closely related purpose of this paper is to describe, in the light of research data, the most significant factors which contribute to the initiation, sustention, and progressive

variation of affective qualities of experience as symbolically reported by both musically trained and untrained listeners. In connection with this second purpose we shall endeavor to justify in the light of research data and in the light of certain relevant phases of musical development, the view that qualities of affective experience do not inhere as constants or absolutes in specific tonal structures. A tentative conclusion will be drawn to the effect that the quality and intensity of feeling aroused by a given tonal pattern is relative to a complex of factors, and that the experienced feeling varies in some manner whenever the complex varies by a significant amount in one or more of its affective stages. For convenience in description, the complex of factors is classified into (1) the adequate stimulating tonal environment; (2) the neurophysiological status of the listener at the time of stimulation; and (3) the psychological status of the listener at the time of stimulation. The tonal environment to which the listener is subjected can be objectively controlled and meaningfully measured within certain prescribed limits. Because of the inadequate means available for measuring objectively the functional efficacy of neurophysiological factors, the neurophysiological status of the attending organism in terms of processes correlative with the affective aspect of experience can be determined only in abstracted part and that part approximated only at some point within a wide range of efficacious possibilities. The psychological status of the total complex can be known only through introspective judgments and through rational inferences based on central tendencies in symbolic behavior. The affective aspect of experience is customarily made intelligible through the medium of verbal communication. Since we can not depend on isolated physiological measurements as criteria of this abstracted phase of experience, our immediate hope in solving the problems set before us rests in the naive yet valid faith that a general agreement in verbal signs is a satisfactory index of the existence of some quality in consciousness common to the lives of many. That our immediate hope is not a bright beacon but only a feeble flicker in a stormwind of doubts and uncertainty will become obvious shortly.

Affective character in music, then, is usually described in those

terms which have been selected to represent those qualities of private experience commonly associated with feelings and emotions. We conventionally speak of music as being pleasant or unpleasant when in reality we mean by our terms to refer to qualities of private experience which the music arouses in us or is most likely to arouse in us. Our daily practice of designating, identifying, and comparing qualities of strictly private feeling by means of words is fraught with serious dangers. Fallacies in this practice loom large whenever we are called upon to objectify the operatives or referants of the words which we so glibly utter.

From the psychophysiological point of view, the problem before us loses many of its inoperative verbal boundaries and almost fades into some meaningless pursuit when we learn that the chief characteristics of affective experience as revealed by self-examination are vagueness, indefiniteness, diffuseness, unlocalizability, and lack of objectivity. Experimentally, we know that similar physiological data may accompany radically different verbal reports in inner experience and we know that considerable uncertainty is frequently attributed by a subject to the word which he may select to designate a quality of inner experience of the existence of which he is certain. Of this we seem to be certain, namely, that we can never be wholly certain that a common symbol which, through the process of introspection, may be assigned by two different subjects to their respective qualities of inner experience, operates to identify these qualities as they actually exist. We can never match directly our own private experience with that of another person, and for this reason we can never be absolutely certain that two persons who use the same word in reporting their separate qualities of experience, actually experience identical qualities. Moreover, we can never be absolutely certain that a given subject has the same quality of inner experience on successive occasions just because he uses the same verbal report on both occasions. His verbal report on either occasion may be the product of inaccurate reference or false recognition. Whenever the self-analysis of an inner experience assumes the form of retrospection to transient, rapidly shifting tonal patterns, the reliability of verbal reports is likely to decrease by a relatively enormous

amount. Hence, judgments made in response to static chords or small tonal fragments are difficult to compare in quality with those judgments made in response to a continuously moving and changing pattern of music extending over several minutes of experience. Finally, when we are forced to admit the possibility that the verbal report of a listener may not operate in experience nor have any meaning whatsoever beyond the mere sounds which the verbal report conveys to an examiner's ears, the problem which we have set before us apparently ends in confusion and completely vanishes for all its initial seeming worth.

Were we to accept such complete annihilation of our problem through the semantic analysis of the terms of reported experience, we should be obliged to annihilate innumerable problems in many fields of research. Part of the foundation of scientific method rests on the statistical validity of verbal reports which symbolize critical observation under experimentally controlled conditions. To discard the verbal report from scientific methodology means the eventual collapse of that methodology. "What do you see?" asks the scientist of an observer, and with confidence the observer reports the nature of his experience by means of a verbal sign. "How do you feel?" asks the medical doctor of the patient, and with unquestioned faith the patient reports verbally a certain quality of pain which he is experiencing. On the basis of such verbal reports the medical doctor may fashion his diagnosis. It is true that the patient may be exaggerating his personal condition, that his verbal signs of an inner experience are poorly selected to match the physician's level of understanding and mode of interpretation, or that his inner experience is far more intense and more complex than the verbal signs indicate. But in spite of likely inaccuracies and inadequacies in the individual selection of verbal signs, it is important to note that in the long run of medical practice, the verbal report of a disordered experience on the part of the patient has proved of practical and life-saving value, and that without the verbal sign as an indispensable tool, medical science could not have reached its present limits of successful achievement. And so in the light of the practical values and accomplishments which overt verbal signs of implicit, private con-

ditions have yielded in other fields of intellectual endeavor, we no longer hesitate to resurrect the material form of our ghostly problem of affectivity and to instill within it a spirit of new meaning.

We shall grant wholeheartedly that as a method of investigating the substantial relationships between a living organism and its environment, introspective analysis expressed in verbal signs has repeatedly proved both extremely variable and unreliable, and for this reason it has been avoided as much as possible by scientific psychologists. But, oddly enough, the majority of psychologists who have investigated the nature of affectivity to tonal stimuli have resorted to and depended upon the variable and unreliable method of introspective analysis. In all fairness to these investigators, let us not censure them too severely for having adopted the more questionable procedure. This common faith in the efficacy and validity of introspective analysis is not a blind faith, nor is it to be construed as an index of indifference to the objective approach in laboratory methodology. This faith in the verbal sign of an inner experience is, if you please, a *necessary* faith—made necessary by the very elusive character of the private data for which search is made. It is a faith just as necessary in musical aesthetics as it is in medical science.

However, no scientist is willing to depend on a single report of an abstracted phase of experience as a means of drawing a generalization about human nature. Nor is any scientist willing to establish the validity of any verbal criterion on the basis of meager, scattered, and poorly organized results. The problem of musical effectivity, as we see it, is just one of the many problems of response statistics. Carefully controlling our stimulus conditions and selecting our listeners on the basis of qualifications for which they have been tested and by means of which they can be functionally classified, we proceed to collect from both musically trained and untrained subjects many thousands of verbal reports for the purpose of analyzing these statistically. From a thorough statistical analysis we are able to demonstrate a community of response to a selected pattern of sounds, and to show within what statistical limits this community of response tends to vary.

When musical affectivity is studied from the statistical point of view in which central tendencies of verbal signs or other patterns of symbolic behavior are described, one of the most prominent features generally to emerge from analysis is that of variability. Both trained and untrained subjects vary their responses considerably over a period of time, even under conditions in which the stimulus is kept very constant. A listener today may describe his feeling to a minor structure as one of sadness; tomorrow, believe it or not, the same listener may describe his feeling to the same structure presented in the same manner, as one of joy. In any attempt to arrive at the meaning of a shift in verbal report to a specific tonal pattern, it is not sufficient to limit our interpretation by the pitch intervals which determine the modality of a tonal structure. We must not forget that the human being is a fluctuating organism and that an unknown shift in the unobserved physiology of the organism or a change in the character of an accompanying train of associated ideas may be responsible for bringing about a radical alteration in the nature of one's feeling tone or affective judgment. Experiment reveals to us that the relationship statistically between an organism's measured physiology and its report of a specific quality of affective experience is neither a constant nor an absolute. Moreover, experiment reveals to us that the relationship between an organism's measured physiology and a specific mode of controlled tonal stimulation, is neither a constant nor an absolute. When the physiology of the organism is neglected in our analysis so that we know nothing of the status of physiological functions at the time of stimulation and response, whatever dependence of the response we may ascribe to physiological changes must remain on the plane of speculation and weak inference. When we examine the statistical relationship between the verbal sign, which of necessity we take in good faith as a certain index of inner experience, and the specific mode of controlled stimulation, we are perhaps no better off than before. Depending upon the type of listener and his level of musical training and appreciation, we can expect different listeners of the same or different types to respond differently on successive occasions when presented with the same mode of tonal stimula-

tion. This fact need not surprise us, since the human organism as an adaptive mechanism is not static but continuously dynamic and fluctuating. We find, then, that the relationship expressed statistically between the verbal sign of an inner private experience is neither a constant nor an absolute in adaptive adjustments to tonal stimuli.

When we endeavor to ascertain the consistency of affective judgments to experimentally determined changes in one or more of the sensory attributes of a given musical configuration, our problem becomes enormously more complex and our ability to predicate the nature of any single judgment decreases accordingly. If the pitch relationships within a given tonal structure are kept constant and either the intensity or durational values shifted, we can expect some shift in affective judgment. A shift of another order may occur when the intensity and durational values are kept constant and either the interval distances or the timbre values are changed by some known amount. Thus we find that the status of an affective judgment is not only dependent upon the physiological status of the listener and his peculiar frame of mind, but also relative to the specific relationships between the sensory attributes which constitute the tonal structure as perceived. To illustrate this latter point, we learn from experiment that for both musically trained and untrained subjects there is no fixity of feeling tone intrinsic to a given chordal combination such as the major triad or the minor triad in either fundamental or inverted position. It has been found that intensity changes may radically affect the complexion of a harmonic configuration. A loud configuration is rarely reacted to as soothing; while a soft configuration is frequently judged as soothing. The character of sadness which in theory has so often been considered intrinsic to the minor structure may be completely masked by changes in intensity, time, and movement. The modal properties considered purely from the standpoint of interval relationships are abstracted properties and, as such, do not operate in response independently of other sensory attributes. The position once formerly entertained by certain theorists, that the minor structure has within it the potentialities to induce feelings of grief and sorrow is no longer

supported by experimental results. It has been shown experimentally that reaction to harmonic configuration is variable for both trained and untrained subjects, and that the mode in which a composition is written when considered apart from other influential tonal attributes has little relation to the type of feeling which the composition may arouse. Compositions written in the minor mode may be reacted to by both trained and untrained subjects as bright, happy, joyful, cheerful, and exuberant, whereas major compositions may be reacted to as gloomy, sorrowful, sad, plaintive, and melancholy. Mood fluctuations and general temperamental differences prevailing at the time of judgment provide for variations in interpretation. In some few instances, the nature of the affective judgment in relation to a given mode is largely dependent upon training to react in a specific manner to a purely intellectual discrimination.

When the responses to major structures are compared with the responses to minor structures under the same controlled conditions of tonal stimulation, it is most important to observe that while variation in individual judgment occurs from time to time, minor structures played softly are judged to have a soothing effect more frequently than major structures played at the same intensity. In comparative evaluation of the two structures, the minor structure loses its soothing qualities when it is played more loudly than the major structure at the same pitch level. Whether the soothing quality of affective experience is more easily induced by a minor third than by a major third, independent of any intellectual discrimination between the two forms, has not been conclusively determined by experiment. In the case of trained subjects who give fairly constant reactions of joy and sadness to major and minor structures respectively, it has been shown that for the most part the judgments of feeling are posteriorily based upon intellectual discrimination between tonal configurations. The subject who has been trained to discriminate between tonal structures differing from each other in interval content has a tendency, first, to label in judgment the modality of the structure, and, second, to describe the affective quality of the structure in those terms which he has learned as the traditional characteristics.

In future investigations planned to reveal any intrinsic properties of the musical modes potent to arouse specific affective states, every serious attempt must be made to eliminate those sophisticates who, by an act of rote memory, apply fixed terms from a learned, traditional vocabulary of affective qualities to the products of their primary intellectual discriminations of tonal form.

Whenever a given chord becomes an integral part of an inter-related succession of tones and chords, the psychological status of the chord is not only determined by the perceptual, ideational, emotional, and motor phases of response that may enter by varying degrees into the reception of the tonal succession, but by the dominance or obscurity that is rendered the chord through its position in the pitch and time series. Again we are confronted with the fact of musical relativity. A chord as part of a dynamic musical pattern is not an absolute value to the recipient. The same chord in a given pattern presented on successive occasions may be evaluated differently at different levels of experience. The fact that a given chord is found to assume a dominant position in musical passages designed to express excitement is no justification for labelling the chord as a chord of excitement when abstracted from the musical passage. As Dr. Hevner and others have recently shown, the perception of a succession of chords as a dynamic musical pattern potent to engender strong emotionalities is not to be identified with the perception of segregated and unrelated chords. The tendency to ascribe definite affective values to various combinations of tones sounded simultaneously seems to have as its source of origin the point of view that affective qualities, regarded as complexes of sensorial elements subject to analysis, show a point to point correspondence with the mathematical structure of the tonal combination in question. This point of view has led to static harmonic analyses which have greatly retarded our understanding of the progressive change of human response to tonal patterns. Any absolute value ascribed to either musical form or musical response can be but one transient, arbitrary point in a long, evolving series of human values. The only absolute which has enduring value is the fact of relativity of human affectivity to music.

PÀRT VI

ABNORMAL PSYCHOLOGY

TOWARD A PRACTICAL CONCEPT OF NEUROSIS

KNIGHT DUNLAP

University of California

One of the curses of psychology for more than 2,000 years has been the confusion of meanings of important terms. Terms with sliding meanings, such as: "unconscious," "sex," "personality," and "intelligence" have misled many laymen and pseudo-psychologists, and not a few psychologists. Terms which mean one thing at one moment and something else at another moment promote the concoction of theories of less ambiguity. These sliding terms, moreover, have been extremely helpful in the building up of rackets for the fleecing of the ovine public.¹

Some of the terms which are widely used by psychologists and pseudo-psychologists have never had any definite meanings. One of these is the term "emotion." Certain other terms have had relatively precise meanings in the vernacular, if not in the dictionaries, but are becoming completely demoralized through the attacks of psychologists and others. "Motivation" appears to be one of these prostitute terms.

Behind this chaos of meanings, which is, in part, responsi-

¹ Rackets based on personality tests, vocational tests, and mental hygiene, which have given these names the characteristic ascribed by Heraclitus to ghosts in the underworld, are, like the psychoanalysis racket, based largely on the ambiguity of terms. Although intelligence testing has its honest aspects, it is no secret that the huge profits extracted from the sale of copy-righted tests has led to extensive racketeering. The most entertaining of the rackets, however, is based on the fact that the ambiguity of terms is a serious evil, together with the desuetude of formal logic. This racket is "General Semantics", which offers cures for everything from falling arches to war, without making any demands on the mental abilities of its clientele.

ble for the weltered condition of psychology, lie two contributory factors.

(a) Psychological terms have, for the most part, developed outside of psychology itself. The vernacular has produced most of the terms; but the effort of philosophers, physiologists and psychiatrists, with psychological ambitions and without psychological background, have added much to the confusion. The psychologist today is still in the position of a watchmaker, attempting to operate with tools contributed by carpenters, plumbers, machinists and farmers.

(b) Psychologists themselves, along with pseudo-psychologists, often appear to be confused by the etymological derivation of terms. For a considerable number of terms, indeed, we accept the fact that the derivations give no clues to the meanings. We are aware that our word "dynamic" means almost the exact opposite of the Greek word *dunamos*. We realize that the several most frequent meanings of our word "intelligence" have no relation to the meaning of the Latin *intellegerere* and its cognates. We are becoming reconciled to the fact that our word "biology" has a meaning which was completely excluded from the Greek word *bios*, although the tendency of psychologists naively to worship psychological theories of modern "biologists" may be due to a dim notion that "biologists" are really experts in regard to what the Greeks called *bios*. The etymologies of some of our other terms, however, appear still to deceive some psychologists.

The term "emotion" furnishes an example. Aside from the notion that the derivation from *e* and *movere* has a holy significance, there would seem to be no explanation for the "breakdown" theory of emotion which restricts the term, formally at least, to unhappy and unfortunate emotions, branding emotions of the happy and fortunate type as no emotions at all, in spite of the fact that those who accept the "breakdown" definition do, like every one else, call these emotions when they forget their definition.

The terms "psychosis" and "neurosis" appear to give psychologists the maximal difficulty, and to impress on some psychiatrists the necessity to justify the classification of the names of

various disorders under the one or the other of these two headings. The invention of the absurd terms "psycho-neurosis," and "neuro-psychosis" had no discernible cause except in the explicit or implicit notion that the etymology of the two words (neurosis and psychosis), is a determinant of their present day meanings. The most absurd of all distinctions: that a psychosis involves the whole personality whereas a neurosis involves only a part of the personality, is probably also due to the etymological deception.

Ten or fifteen years ago, it was possible to solve the problem by pointing out that the etymology of the two words must be disregarded. That whatever the history of the usage of the term "psychosis"; what it may have meant to early alienists; and why it had that meaning; have little bearing on its meaning today. That, however unfortunate was the introduction of the term *neurosis* by Charcot and Janet, it does not mean a nervous disorder as contrasted with a mental disorder. To practical psychiatrists and abnormal psychologists the two terms were merely names for two groups of disorders, and could be explained only by enumerating the disorders which, by convention, are included under the two heads. In spite of this solution, however, there are texts still in use which define a "psychosis" as a mental disorder, and a "neurosis" as a nervous disorder, although the actual relation of the two terms is more nearly the reverse of this pseudo-etymological relation.

Ten years ago it seemed possible to define a psychosis as a mental disorder which is the syndrome of an organic lesion, deterioration, or dysfunction; and to note that the physical condition is the real disorder. The organic psychoses, then, could be defined as those psychoses for which the organic bases are definitely known. The functional psychoses accordingly, were those syndromes which we *believe* have organic bases, but for which the physical bases are unknown. Neuroses were left as disorders which are probably systematic bad-habits, not caused by organic lesion, organic deterioration, or organic dysfunction, but resulting from normal function under unfortunate conditions.²

² By the use of the terms "dysfunction" here we sidestep a terminological problem which is associated with a theoretical confusion. The word "malfunction," which

Today, this rationalization is impossible. The organic psychoses remain as they were. But the line between functional psychoses and neuroses seems to break down. We admit that the term "dementia praecox" (or the more cautious term schizophrenia), represents no entity, but is merely a waste-basket class, in which are thrown disorders of various kinds which cannot conveniently be classified elsewhere. Nevertheless, the fact that patients of certain types, classified as "dementia praecox," recover when given the same treatment as others classed as "neurotics," is disconcerting. On the other hand, the cases classed as neurotic, which turn out to be cases of Brucellosis, or of one of the other types of undulant fever; and which accordingly would seem to belong in the class of organic psychoses; are equally disconcerting.

Epilepsy, obviously another waste-basket, including a variety of different fundamental disorders which agree in a single symptom, begins to be productive of difficulty. I am speaking of course of "idiopathic" epilepsy; not of the range of other epilepsies, known to be syndromes of definite organic disorders. Some of these cases yield to treatment based on the supposition that they are primarily (note the word "primarily") neuroses; that is, systematized bad habits. The modern tendency, in fact, is to discard the word "epilepsy" and substitute the term "convulsive disorders." We will probably, indeed, shortly be able to eliminate the deceptive term "epileptic personality" and the artificial concept it designates.

The increased knowledge that the old psychiatric terms are not satisfactory seems to have led to a tendency to take liberties with the terms; liberties which are not conducive to progress in the understanding of the disorders to which the terms are applied. Convulsive seizures in rats, which have been problems for

we here avoid, has an ambiguity which we can do nothing about, at least in any simple way. Stammering, for example, is often referred to as a malfunction, although it is the result of normal response tendencies under assignable environmental conditions. (Any child which is normal in its development up to two years of age can, by appropriate treatment, be made to stammer, although possibly some mentally defective children cannot be made to stammer.) In this paper, we intend the term *organic dysfunction*, to indicate a condition in the operation of non-neural tissues or organs (*e.g.*, the endocrine glands) which conduces to formation of undesirable response habits.

animal psychologists for nearly 40 years, have received publicity as "experimental neuroses," thus threatening to obscure the problems of abnormal behavior of rats and other animals, and offering no aid towards the solutions of problems of human neurosis. The explanation offered for this confusion has been that since the word "neuroses" has no fixed meaning, one can call anything one pleases a "neurosis." I bring up this matter which, indeed, recent writers have clarified, to emphasize my point, that, in spite of the confusion in the concept of neurosis, it is by no means devoid of meaning, and that our problem is to clarify, not to increase the confusion. The confusion of convulsive disorders with neurosis is especially unfortunate; for although some cases of convulsive disorder probably *are* neurotic, we cannot assume that in any of these cases the neurosis is the whole disorder, and there are undoubtedly many cases which are strictly organic psychoses, in their inceptions at least.³

My positive suggestions are not results of experimental work on man or animals, but of clinical work in which I have been engaged for a number of years. During the development of the principles which I suggest, my co-workers and I have been impressed by the increasing number of cases which have been cleared up. Nevertheless, I endorse fully the statement of Janet, that no piling up of case histories can ever prove a therapeutic theory. The only purpose of putting out conclusions from clinical work is that others may consider them, and check them in their own clinical practice.⁴

³That the patient suffering from "epilepsy" tends to become neurotic, whatever the cause of the convulsions, particularly if he is given treatment, is another matter.

⁴To those who understand the magnitude of the importance of *praxis*, as contrasted with *science*, it is sufficient to point out that progress in clinical work is largely improvement in *praxis*.

⁵The suggested word *thumosis* is derived from the Greek *θυμος*, which, for our purposes is best transliterated *thumos*. In Homer, *thumos* is the only term used for the ego, I, or center of conscious life; and apparently this continued to be the standard term down to Plato, who, (or preceding unknown Pythagoreans), consolidated the psyche, in its two meanings of life stuff and ghost with the *thumos* and the *daimon*, calling the synthetic product psyche: a result which confused psychologists from then on. In the considerable number of English words derived from the same root as that of *thumos*, some transliterate the root through the

I have the conviction that in the future, classification by causes will be more important than classification by syndromes. I strongly suspect, moreover, that no condition worthy of being called a mental disorder or a serious maladjustment, is ever due to mental causes alone. In the organic psychoses, presumably, we have a group of disorders which are the result of physical (organic) causes alone. This presumption may be wrong. The deterioration in paresis may be ascribed to organic lesions. But in no case does the syndrome appear to be merely deterioration; and the mental disorder and the syndrome of symptoms, in the organic psychoses, are identical. In spite of this faith, and this suspicion, I think that the concept of neurosis is a useful one. Perhaps we should abandon the old name, and substitute another one. I have, indeed, suggested *thumosis*⁵ as a possible replacement term; but for the present, we may well retain the old name, and seek to improve the concept it designates. For convenience I will present in a brief scheme the points at which my coadjutors and I have arrived.

1. We distinguish between *perturbation* and *neuroses*. Any person may be for a period of minutes, hours, or even days, deeply disorganized, in any one of a variety of conditions. We consider a neurosis, on the other hand, to be a condition of relative permanence, transferable to conditions other than those in which it was produced, but not necessarily displaying in its syndrome a fixed set of symptoms.

2. We have reason to conclude that no sudden emotional shock, or mental stress of relatively brief duration can produce a neurosis. An emotional shock to a person in process of developing a neurosis may bring out symptoms which were not displayed previously; but in actual cases, careful search of the history usually discloses the fact that the appearance of the symptoms was not so sudden as the stories of parents and friends reported. A typical

Latin as *thy-*; and it is to avoid confusion of meanings with these words that the direct transliteration *thu-* is suggested. In making up a new term, it is always interesting to derive it correctly from the appropriate Greek or Latin root—and then see what happens to the term.

An alternative term I have earlier suggested is *thumo-ataxia*, the derivation of this term indicating the meaning proposed for it.

case is that of a young man who "began to stammer" after a severe fall in which some bones were broken. The fact is, that the fellow had been stammering for several years. The fall did, however, aggravate the symptom somewhat. Such tales and their lack of foundation illustrate the saying of the poet that: "*Heaven lies about us in our infancy, and our mothers lie about us the rest of our lives.*"

3. The common maladjustments, with symptoms such as stammering, tantrums, apathy, inability to make social contacts, etc., are not distinguishable from neuroses; either on the basis of causal patterns, or by the syndromes of the disorders. We might continue to apply the name "maladjustment" to neuroses which do not completely incapacitate the patient for vocational and social life; but the criterion implied here is a fragile one. Use of this criterion is equivalent to saying that a serious maladjustment is a neurosis and a slight neurosis is a maladjustment. The distinction depends on the meanings of terms such as "slight" and "serious" and "incapacitation"; which are relative to interests of the diagnostician, and to the sort of life. A disorder which unfits one man for the life of a plumber may not unfit another for the life of a lawyer, and vice versa.

There appears to be a wide range of disorders, the syndromes of which are so similar that the disorders are worthy of a common or class name. So many of these disorders are such as have long been described as "neuroses," that this class name seems appropriate. That some types of disorder belonging to this group have customarily been classed in other categories, particularly in the waste-basket of dementia praecox and epilepsy, does not alter the situation. It is simplest to class these cases with those with which they have affinities, at present, under the name *neurosis*.

4. It is no longer sensible to search for "the cause" of a neurosis, grave or slight. Both diagnosis and therapy are demoralized unless we seek, in each case, for a *pattern of causes*; which may be, in some cases, a rather complicated pattern. While it cannot be demonstrated that there are no neuroses with single causes; the constant failure to find any cases which are clearly mono-etilogic makes it imperative to look always for multiple causes.

We find it worth while, also, to distinguish where possible, the several types of causes involved in the causal patterns. In especial: primary; predisposing; precipitating; sustaining; and symptom-determining causes, need to be discriminated carefully. Mistaking these causes may make diagnosis incompetent and therapy ineffective.

5. We find it essential to seek for both "mental" causes and "physical" causes in the causal pattern. Cases in which the causes are exclusively of the one sort or the other are few, if indeed there are any such. Thorough medical diagnosis is necessary in the majority of neurotic cases, and should be had in all as a routine measure. The securing of a competent diagnosis is not always simple, and past diagnoses by unknown physicians must be disregarded. Failure to have a medical diagnosis made has been responsible for many of the failures of psychiatrists in private practice.

6. Among the common features of the disorders we class as neuroses, the most conspicuous is the introspective tendency. We are inclined now to say that no disorder should be called neurotic unless this central symptom is evident. On the other hand, any person who forms the habit of continuous self-examination is definitely in danger of becoming neurotic. This does not mean that the introspection is a primary cause of neurosis, but it may be a predisposing cause, and often is a sustaining cause. That a symptom of disorder may be a causal factor also, must be borne in mind in any work with neurotics.

The introspective tendency which is a central symptom in neuroses, should not be confused with the so-called "introversion" of popular psychology. Tests for "extraversion-introversion" do not detect it, and persons who come out as extraverts on the tests may be sad neurotics. While it might be a good thing if the various current definitions of so-called extraversion-introversion were discarded, and the terms related simply to introspection and its absence, the confused meanings of these terms are so well established, and so useful for the commercialized personality racket, that this is impossible. The clinical psychologist has no alterna-

tive to just forgetting about the mythical sheep and goats called "introverts" and "extraverts."

7. Recognition of this habit of self-examination as the vital neurotic characteristic, has explained findings which by themselves are puzzling; and has accelerated progress in these findings. The fact that the use of test-forms, or questionnaires to be filled out by the patient, produce fraudulent information; the fact that routine questions and answer usually succeed merely in convincing the patient of something about himself which isn't true; the fact that any questioning of, or skeptical attitude towards, the patients' statements in the early interviews, is fatal to competent diagnosis; these findings are compatible with the introspective habit of the patient. The violent concern of the neurotic patient about his happiness and unhappiness is in part an inevitable result of self-examination; in part it is another side to the same thing. The neurotics who have been labeled "extraverts" display this hedonistic pathology just as strongly as do the neurotics who are so-called "introverts."

8. In interviews with a neurotic patient, he is required to introspect, in order that diagnostic information may be obtained. This procedure, in itself, will make the patient worse (if possible), unless some counteractive technique is included. I am inclined to think that the bad results of psychoanalytic treatment are in part due to the increase of the neurotic condition in this way. Most of the patients who are encouraged to increase their self-examination appear to come through the man-handling fundamentally worse, although the symptoms may have been recast so that the person appears improved. As a matter of fact, reorganization of symptoms is often a benefit, just as the transfer of a boil to the shoulder from the rump might make life easier for the patient.

The solution of the problem of examination of the patient is a form of negative practice. The patient, understanding that self-examination, or self-evaluation, is the deadly symptom, and also a sustaining cause, is brought to look on the introspection required in the examination room as damaging in itself, but temporarily necessary. I find it useful to tell most neurotics that in-

trosection in the examination room is comparable to the opening of the abdomen for the treatment of appendicitis; a lifesaver if the opening is expertly done. On the other hand, I tell them, the self-observation of the patient, carried on constantly, is comparable to having a zipper put in the abdominal walls, so the patient can unzip every few minutes and inspect his entrails. Getting these ideas before the patient, of course, is not the whole story.

9. The prophylactic methods against the introspective habit involves another important point in the treatment of neurotics, and the prevention of neuroses. Popular advice given to neurotics is, in many cases: "*Use your will power.*" This advice often is given by persons whom the patient supposes are experts in mental treatment. Very recently I have seen a girl who has stammered for 12 years; one of the rare female stammerers who are really good at it. In a clinic of national reputation where she was kept under observation for arthritis, she was told, on being dismissed, that her trouble was largely psychological, and that her stammering was a part of it. She was told that she would be improved if she would just stop stammering. Simple as that.

All neurotics, confirmed or incipient, have to be told that will-power is totally ineffective, so far as breaking a habit is concerned, whether the habit be that of stammering, worrying, or masturbation. Exerting effort (which is what is meant by "will-power" in popular verbiage) to cease introspecting is just more introspecting, *plus*, of course, some muscular effort. The number of patients suffering from organic disorders such as undulant fever, diagnosed as neurotics, who have been told, "*all you have to do is to snap out of it,*" is large, and these cases are only clearer illustrations of the futility of will-power than are all other neurotic cases.

This will-power nostrum, of course, goes back to William James, who did not invent it, but who certainly backed it powerfully, in his expressed and implied prescription for breaking habits. If you have the drinking habit, and want to break it, it's simple. Just stop drinking. Of course, every patient knows that the way to get rid of a habit is to break it; what he needs is help

in the procedure of breaking it. Breaking the habit of introspection cannot be done by "effort of will" but can be accomplished by psychological methods which start with the explanation to the patient of what the bad habit is that he must break. The further procedures are so well known to many psychologists, and so incomprehensible to others, that it is useless to attempt even to outline them in a brief paper.

10. We are increasingly of the opinion that great help will be given to the study of human neuroses by the careful study of abnormal conditions in animals. More detailed control of experimental conditions, and more careful consideration of the actual syndromes displayed by the animals subjected to these conditions, are imperative. In particular, the question of transfer of abnormal conditions from one situation to another; the period of continuation of the disorders; and the process of recuperation; need minute study. Research of this type has been begun in several laboratories, and should be forwarded and supported.

Experimental work on human beings may be impossible. It is possible, however, greatly to improve the clinical studies. Studies based on classification of cases by routine psychiatric methods have proved sterile. Misuse of the results of statistical treatment of data has been too prevalent. Glaring examples are to be found in the so-called standardization of tests for extraversion-extroversion, which turn out, on critical examination, to be gross deceptions. These, and other so-called personality tests, however, are not consequential, since even if these tests could be standardized in any true sense of the term, their use in research with neurotics would still be an obstruction rather than a help. Where the experimental method is inapplicable, no substitute of a pseudo-experimental sort is admissible. We should recognize that the clinical method itself is capable of being a real research method, and should depend on this where experiment is not possible.

In attempting to arrive at a useful concept of neurosis, or whatever we finally decide to substitute for that name, we are faced, as I have tried to point out, with a problem which is completely practical, and which can be solved only by clinical re-

search. It is not possible to detail, in a brief paper, the whole tissue of methods of diagnosis and therapy which have opened up the possibility of such research. The features of procedure which have been forced upon us by the work so far include: (*a*) Abandoning of armchair theories, including some of my own. (*b*) Disregarding etymological derivations of terms, and eliminating confusions growing out of pseudo-etymology. (*c*) Junking traditional methods of diagnosis such as the use of questionnaires and formal quizzing, so dear to the hearts of some psychologists of the mechanical routine type; and adopting better methods. (*d*) Avoiding bondage to traditional classifications of mental disorders, and recognizing that certain cases conventionally classed as schizophrenic, or as convulsive disorders, are really as much neuroses as are neurasthenia, hysteria, and other disorders conventionally called neuroses. (*e*) Recognizing, too, that certain cases commonly called neurotic may also legitimately be described as organic psychoses. (*f*) Paying more attention to patterns of causation, and ceasing to look for simple causes. (*g*) Recognizing the probability of both organic and mental causes in the pattern of most neuroses. (*h*) Working more critically on problems of abnormal behavior of the lower animals. (*i*) Guiding research by psychological principles, instead of by antiquated psychiatric assumptions and tests.

PSYCHOPATHY AS A PSYCHOLOGICAL PROBLEM

ROBERT M. LINDNER

United States Public Health Service, Lewisburg, Pennsylvania

Perhaps the most challenging enigma in clinical psychology and psychiatry today is posed by those individuals who are diagnosed as psychopathic personalities. As the voluminous and often contradictory literature shows, the single fact of the prevalence of such persons in our society—taken together with the ramifying potentialities existing in this variant of personality deviation—poses a complex and fundamental problem.

Unlike other so-called mental disorders and defects, psychopathy is difficult to lay hold of, to grasp and subject to the usual techniques of clinic and consulting room. For one thing, it lacks a central theme or rallying point for its various and diffuse symptoms. It has no core, no marrow: it is amorphous, elusive.

With schizophrenia the clinician can usually pyramid his findings on the bed-rock of affective impoverishment; with the paranoiac the fulcrum of systematization shapes a comforting handle. In the affective psychosis the cyclical or fixed mood-patternning sounds the dominant chord. Whether psychopathy simply lacks such a centrum as other disorders possess; whether our regard is not sharp enough to detect it; or whether it is in the nature of the thing and the essence of the diagnosis that no such magnetic point exists, are questions that must eventually be clarified.

Hydra-headed and slippery to the touch though it is, socially regarded, psychopathy represents the most expensive and most

destructive of all known forms of aberrative behavior. Its afflicted crowd our prisons, harass our police, distract our teachers, enervate our parents, impede our industry, annoy and delay our military. From an economic point of view alone, its consideration for a prominent place on the research schedule of the scientist in human behavior is justified.

I

Psychopathic personality is an anachronism, and herein lies its problem for the psychologist. Strictly speaking, were it not for clinical experience which teaches above all that anything is possible, there is a strong temptation to dismiss the whole thing as fantastic and improbable; to declare that no such admixture of traits and tendencies could exist within the confines of a single organism. To illustrate this apparently capricious statement and to demonstrate wherein the riddle of psychopathy lies, let us examine a limited number of the classical signs of this disorder.

A — INTELLIGENCE

The intelligence of most psychopaths is in the range from average to above average. Now intelligence is usually conceived of as acting somewhat as a brake upon behavior. Presumably, the higher the general intelligence the greater the potentiality there exists for grasping such concepts as social obligations, property, the value of human life, responsibility, etc. In addition, high general intelligence should, theoretically, provide foresight and an ability to weigh consequences. Even specific intelligence such as manual dexterity at least provides minimal insurance that a bridge between the individual and the community can be built on the basis of mutual exchange. Assuming now that all other things are equal; that is, that psychopathy *is* a clinical entity and its resultant modes of behavior not attributable to neurosis or what is usually meant by psychosis, the problem is why this segment of humanity cannot function in such a manner as to evidence comprehension of the abstractions basic to social living and prophetic for the individual's welfare.

The old English term, moral imbecility,—now revived as

moral deficiency in the latest *Standard Nomenclature of Diseases* of the American Medical Association—is perhaps the most aptly descriptive manner in which the matter has been phrased. The difficulty, however, lies in the semantic implications of the term with its undertone of segmental or facultative psychological bias.

One direction in which an answer to the riddle posed above may, perhaps, be found is hidden somewhere in the fact that there is, as our popular usage of language testifies, a typology of intelligence; that is, another dimension to the “global” *etwas* the word intelligence seeks to comprehend. Such a typological factor is hinted at by the words “cunning” and “shrewd”. It is quite likely to be the case that this dimension is inaccessible to the gross instruments which serve us as measuring devices in psychometry today. We have been concerned chiefly with obtaining measures of intellect in terms of amount but have utterly neglected the dimension of kind or type. An Einstein, for example, may measure in the upper reaches of the slide-rule so far as potential intelligence is concerned or so far as ability to manipulate currently employed test-items is considered: but on a scale of—let us say—“cunning” (in the accepted popular sense of achieving an end through devious and often socially unapproved means) it is quite likely that his rating, his cunning-quotient, would be low. Now it may well be the case that the psychopath, average and over in test intelligence, possesses a different *kind* of intelligence and thus achieves the appearance but not the reality of equation with the rest of the population. This kind is perhaps of an order precluding comprehension of those factors which enable men to exist in communal groups. If this is so, we need from the psychologist a broader definition of intelligence to account for its typology, and, for psychopathy in particular, a description of the *kind* of intelligence shown by psychopaths.

B — EMOTION

No less confounded is our understanding of the affective side of psychopathy. Indeed, we are here confronted with the spectacle of a complete breakdown of our usual notions regarding the emotions.

Descriptive psychology characterizes the psychopathic personality as unstable, immature, and superficial. The clinical picture shows exaggerated, unpredictable response to certain stimulating experiences. It also shows a shallowness of affect that is reflected in an inability to maintain emotional ties and a general immunity to those situations which normally call for an emotional response. So we have the contradictory duality of too much and not enough; both significant features of a single disorder. We view both sides of the coin at the same time. On the one hand, the delicate physico-chemical poise of the psychopath encourages emotive lability; on the other, severe restraints or inhibitions characterize both the emotional display and response.

The problem is a serious one, but it offers two royal roads to solution: the physiological and the dynamic.

Already suggested is the application of the concept of homeostasis to the behavior of the psychopath. With a research attack based on the premise that typical psychopathic manifestations are tensional discharges aimed to restore a disturbed organismic balance, the psychosomatic aspect of the disorder is brought into focus. Since the maintenance of a dynamic equilibrium is a basically inclusive principle of life, it would seem a proposition especially germane to a disorder the crux of which is the chaotic ambivalence of affect. Evidence for a homeostatic view of psychopathic behavior is accumulating from electro-encephalographic, polygraphic and other sources. There is also a wealth of clinical experience depicting the spasmodicity and explosiveness of psychopathic reactivity.

The dynamic view of psychopathy illumines especially the role of emotions by describing ultimate causes for the restraints and inhibitions noted. The conceptions of depth psychology properly weighed and cautiously applied should yield insight to the paradox of too much and not enough by an operational description of such mechanisms as sublimation, repression, and projection. It should help clarify and explain the "blockage" that in the psychopath prevents normal display and response in some instances while encouraging volatility and over-expression in others.

C — SEXUALITY

The perversive nature of the sexuality of many psychopaths is well-known and, as in every other sphere of his behavior, inconsistent, diffuse and confusing. Since he experiences tremendous difficulty in achievement emotional rapport with others, and since permanency of emotional or intellectual ties is impossible to such personalities, his sexual history is adventitious, transient, non-selective. The act itself is a purely biological expression without the sophisticated preliminaries observable among other personality types. It is consummated at white heat with an object of convenience rather than of choice. Something in it suggests a continual searching. There is about it a quality of experimentation, as if with each new love-object something long sought after may perhaps be found. The sex-life of these psychopaths is a voyage of discovery. Because of this, the sex of the love-object matters little and the actual role, whether active or passive, aggressive or submissive, dominant or dominated, is also of no consequence. Like his nomadism, it is a seeking for and a running from.

And yet, withal, the attitudes of such psychopaths are for the most part thoroughly masculine (if a male and feminine if a female). Only in a motor fashion are they bi-sexual.

We shall never answer the sexual riddle of psychopathy without a new orientation as regards homosexuality. This necessitates formulating a basis of distinction between true, haphazard and situational homosexuality and arriving at a real understanding of such nebulous concepts as drives, needs, goals and desires.

D — COURSE

A curious feature of the clinical entity subsumed under psychopathic personality relates to its appearance as a disorder of behavior at an early age, the exacerbation of symptoms in late adolescence and early adulthood, and finally its almost total disappearance from the age of about thirty-six onwards. Very few real psychopaths can be encountered whose ages are above forty, and the writer has never observed a case of psychopathy where the initial difficulty leading to clinical contact appeared after thirty-five. Where manifestations of psychopathy are met with

in an older adult, such signs are frequently by-products and concomitants of other disordered conditions.

Old psychopaths do not exist because (1) their reckless style-of-life has culminated in early and violent demise; (2) the psychopathic symptoms merge with other instabilities and result in different diagnoses; (3) with biological maturity the signs of psychopathy tend to disappear.

All three propositions are undoubtedly creditable. Yet there is a further feature of psychopathy which lends emphasis to the final surmise. Many psychopaths are startlingly youthful. One continually mistakes their ages and is always surprised to learn they are older than they appear. Somehow infused in the general patterning of the disorder (as it appears in such persons) is a preservative that prevents the greying of the hair, the loss of sparkle to the eye, the lack of sprightliness to the walk, the flaccidity of the musculature. This observation is a powerful argument for a further investigation of the physiology of the disorder and a profounder estimation of the psychosomatics involved.

II

It should be pointed out that we clinicians have somewhat of a general idea as to the genesis of the psychopathic state. By the foregoing signs and many others, the psychopath is recognizable. It is only that, scientifically regarded, he is an anomaly, posing a conjoint functional-organic problem. Stated baldly, the psychology of the psychopathic personality is a mystery. On the basis of complete anamnesis the life history can be reconstructed in such a manner as to point up those precipitating experiences and exposures which can be held important in the genesis (so far as we know at present) of the disorder. Yet, even when this has been done, the essence of the condition, its mode of operation, remains unknown to us. Let us examine a case in which many of the precipitants and some of the predisposants have been worked through.

K. G. was born in 1920. He was the fourth of six children. The family existed on the borderland of impoverishment: they occupied a battered frame-dwelling of five rooms in a section neighboring on the docks

of a large city. The father had been a stevedore, was often unemployed, preferred the male atmosphere of the corner saloon to the squalor and noisomeness of the home. He was brutal in his dealings with the children and especially so with our subject. He would often come home in an intoxicated condition, hurl abuse at the mother, sometimes strike her. She, on the other hand, was mild-mannered, overburdened, always ailing and distracted. According to our subject, she suffered a "nervous breakdown" following the birth of the last child. For this she was treated at home by a neighborhood physician who, if our subject recalls correctly, "gave her pills and made her lie down for an hour every afternoon".

K. experienced a normal birth without complications or sequelae. Labor was mild (according to the mother) and of about two hours duration. K. was a healthy boy and had only the usual childhood diseases. For the first year and one-half he slept in the parental bed. Under hypnosis and free association he recalled having observed parental intercourse many times during this and subsequent years. These impressions were strong. He conceived of his father as hurting his mother. When the next child arrived he was evacuated to a crib next to the parent's bed: at three, after another child had been born, he was placed in another room where he shared the sleeping accommodations with two older brothers. There was some sex-play with these boys, involving no more than the mutual manipulation of genitals.

Pre-school career soon demonstrated waywardness. The lack of parental control was only emphasized by the beatings he received from his father on rare occasions when some bit of misbehavior was directly observed. K. says that at other times "the old man beat me just for exercise." During this time he was associated with a "wild crowd" of boys between the age of 5 and 12, the members of which engaged in vandalism and petty thievery.

He was sent to school at seven. He seemed to learn rapidly and had he been more respectful toward his teachers and less of a truant his school history would have been little different from that of other children. Instead, he played hookey, annoyed other children, once struck a teacher; at the age of 10 he was referred to the juvenile Courts but was released on probation to the custody of his parents. After about six months it was necessary again to refer him to the Courts. He was sent to an institution.

Life for K. in the juvenile institution was a series of punishments, escapes, and absorption of decidedly criminal-class attitudes and ideas. He is described as having been a bright, impulsive, wayward, unstable boy with better than average intelligence and a conduct disorder that expressed itself in rejection of authority, aggressiveness and rebelliousness. He was observed in the commission of homosexual acts on at least two occasions. Curiously enough, during one of these he was passive; during the other he was aggressive.

At fifteen he escaped from the institution and, without funds and all alone, spent almost a year hoboing about the country. He had numerous sexual adventures during this period, found it impossible to hold a job, often resorted to thievery to make his way from one place to another. In a midwestern state he was apprehended for the theft of an auto and sentenced to 1 to 10 years in the state reformatory. Here he was examined psychiatrically and the possibility of psychopathy was raised. His I. Q. was 112, serology negative, health excellent. Soon after admission he assaulted a fellow inmate with a knife over some triviality. Fortunately the victim received only superficial wounds and K. was punished with solitary confinement for a short period. His institutional adjustment from that time forward was fair, although he was cited for insolence, refusing to work and other minor offenses from time to time. After having served three years he was paroled.

K. then managed somehow to enlist in the Army. His record was only fair but the major disciplinary offense noted was overstaying leave. Soon after war was declared, K. went A.W.O.L. and after 30 days "just bumming around," turned in at an eastern fort. He was placed in the guardhouse. One night he and his fellow prisoners broke out, knocked the sentry unconscious, and made good their escape. About five days later, in another state, K. was arrested by federal agents for transporting a stolen auto across state boundaries. He was dishonorably discharged from the Army and sentenced to a lengthy term in federal custody.

On admission to a federal penitentiary K. was described as "unstable, egocentric, impulsive and reckless . . . little notion of his social obligations and duties . . . amoral . . . lacking in insight and showing poor judgment . . . selfish . . . boastful and a braggart . . . expressing ideas of hate against his father and all authority as well . . . tense . . . aggressive-minded and demanding." For almost a year he was in continual difficulties with fellow-inmates and institutional officials. It was necessary to discipline him on five occasions for offenses ranging from sheer refusal to work to assaulting a fellow inmate and striking an officer. Finally he was hospitalized for study and observation and eventually transferred to an institution specially equipped for the management and treatment of psychopathic personalities.

Some of his memories are interesting from a dynamic point of view. When he was about four years old he was standing in the lavatory at home. It was after midnight and he had been awakened from sleep by a need to urinate. The door opened suddenly and his father staggered drunkenly into the room. K. recalls the odor of vomitus. His father told him to "hurry up and get the hell out of there before I pull that thing (penis) off." Another memory also deals with his father. This scene took place when he was about 9 years old. He had been sent by

his mother to the dock where his father was working. He was to deliver a message and ask for some money. The men were lounging in the shade of a warehouse when K. arrived. They had been paid that morning and a crap game was in progress. His father was sitting on his haunches, watching the game, perhaps participating in it. K. approached him and began to deliver his message, but before he could do so his father lifted him from his feet and roughly deposited him on top of a large crate at the waters' edge. The men all laughed as his father returned to the game. K., looking down, was frightened and began to cry. He recalls that he stopped suddenly on noticing a "pinch-bar" lying on the crate. He lifted it, took careful aim at his father, then flung it with all his might. It landed near his father's feet. The recollection from here on is unclear. He recalls only that hasty hands lifted him from the crate and a severe beating, accompanied by howls of laughter from the men, was administered. Additional memories of interest include experimental intercourse with a sister during a time when they were both confined to a room because of chicken pox, the theft of small sums from his mother's purse, the time when he was so enraged at the attention paid to a new baby that he purposely hit his head against the corner of a table and cried until his mother picked him up and held him to her.

The case presented above is replete, even in abbreviation, with those so-called mechanisms and dynamisms that are supposed to be basic to mental disorder. Sibling rivalry, Oedipus situation, failure to introject society through Father image, incest guilt, castration fear, symbolic possession of the mother; all of these appear. But why did they lead to the psychopathic behavior patterning rather than the schizophrenic, the depressive, the obsessional neurotic, or any other? And why is it that in the many cases this writer alone has observed, only infrequently has another member of the family group demonstrated the same style and mode of behavior?

Perhaps as a basis of treatment such dynamisms as are shown in our illustrative case are of value. But when it comes to deriving from them a psychology of psychopathic personality; when it is (as it should be) a matter of determining the *modus operandi* of the disorder and understanding it functionally, they are of no consequence.

Psychiatry deals with psychopathic personality on the Kraepelinian level. It is content to describe the symptoms and relegate them to properly provided niches. Since the function of psychiatry

is to treat, this is a harmful procedure only when the psychiatrist deceives himself into supposing that the sorting process he accomplishes does anything more than label and classify for convenience in handling. As for psychologists, theirs is the task to work through the puzzles in psychopathy by the techniques of the laboratory.

REFERENCES

1. CANNON, W. B. Organization for physiological homeostasis *Physiol. Rev.*, 1929, 9. Also, *The Wisdom of the Body*, W. W Norton & Co, N. Y., 1932.
2. CASON, H. (ed) *Summaries of Literature on Constitutional Psychopathy*, 5 vols., (mimeo) Springfield, Mo., 1942.
3. CLECKLEY, H. *The Mask of Sanity*. C. V. Mosby & Co, St. Louis, 1941
4. FLETCHER, J. M. Homeostasis as an explanatory principle in psychology. *Psychol. Rev.*, 49, 1942.
5. HENDERSON, D K *Psychopathic States*, W W Norton & Co., N. Y., 1939
6. KAHN, E. *Psychopathic Personalities*, Yale Univ. Press, New Haven, 1931.
7. KARPMAN, B. *The Individual Criminal*, Nerv & Ment. Dis. Pub. Co, Washington, D. C., 1935
8. KEMPF, E. J. *Psychopathology*, C. V. Mosby & Co, St. Louis, 1921.
9. LINDNER, R M Experimental studies in constitutional psychopathic inferiority. *J. Crim. Psychopathol.*, IV, 2 & 3, 1942, 1943 Also, Homeostasis as an explanatory principle in psychopathic personality, *Proceedings of the 72nd Annual Congress of Correction of the Amer. Prison Ass'n*, N. Y., 1942. Also, *Rebel Without A Cause*, Grune and Stratton, N. Y., 1944.
10. ORR, D W Is there a homeostatic instinct? *Psychoanal. Quart*, 11, 1942.
11. PARTRIDGE, G E Current conceptions of psychopathic personality. *Amer. J. Psychiat.*, 10, 1930.
12. REICHARD, J. D The psychopathic personality: An organic viewpoint, *Proceedings of the 72nd Annual Congress of Correction of the Amer. Prison Ass'n*, N. Y., 1942.
13. SILVERMAN, D. Electroencephalographic studies of criminal psychopaths, *Proceedings of the 72nd Annual Congress of Correction of the Amer. Prison Ass'n*, N. Y., 1942.

PHARMACOLOGICAL SHOCK THERAPY AS A PSYCHO-BIOLOGICAL PROBLEM

GEORGE W. KISKER AND GEORGE W. KNOX

*Columbus State Hospital and the Department of Psychology of
Ohio State University*

The impetus given to pharmacological shock therapy by Sakel (31) in 1933, ushered in a new era in psychosomatic and psychological research. Foremost among the problems which have presented themselves, and which may be legitimately studied by the psychologist are objective criteria by which the results of shock treatment might be judged, and those having to do with the nature of the changes which take place at a psychological level.

The question of personality organization and the possibility of that organization being temporarily or permanently modified as a result of pharmacological intrusion, is one which warrants careful consideration. It is well known that physical agencies can, under certain conditions, in themselves alter personality structure. The entire range of "organic" personalities falls into this category. The paretic, the encephalitic, the cerebral arteriosclerotic, and the post-traumatic individual exhibits a characteristic personality pattern because of the underlying physical modifications of the brain field. Such modifications isomorphically produce phenomenological changes which give the identifying tone to the personality picture. Personalities of this type, however, are highly crystallized and permit a minimum of adaptability. A channelization has taken place which cannot be influenced, to any great extent, either by the individual or by other individuals. Is a similar mechanism at work in the case of insulin and metrazol shock

treatment? Are basic personality patterns modified, and if so, in what direction? If personality structuralization is modified, how are we to account for post-therapeutic regressions to former stages? These, and other, questions persistently confront the investigator concerned with the theoretical aspects of pharmacologic treatment of mental disorders.

Insulin and metrazol therapy, appearing as it did, swiftly and dramatically, left little opportunity for a consideration of the theoretical implications of the technique. Under the circumstances which surrounded the introduction of shock therapy, it is not surprising that the utilitarian aspects of the problem were stressed to the utmost. Recent months have seen an increasing literature bearing upon the relation of the physiological changes, vague though they may be, to the overlying behavioral picture.

The manner in which this behavioral picture is changed as a result of pharmacotherapeutics is a problem upon which little constructive light has been thrown. That change does take place, in many cases, is not to be denied. Anyone acquainted with the course of a series of insulin and metrazol patients has had numerous opportunities to observe progressive behavioral modifications. Often, these modifications are in the direction of more adequate interpersonal adjustments. It is somewhat more difficult to evaluate the nature of the problem of intra-personal adjustment. That these two types of adjustment are not identical, and often completely dissimilar, is evidenced in a number of psychoneurotic and psychotic conditions in which the quality of thought processes is of one kind while the overt behavior of the patient is of an entirely different kind. We see this situation excellently expressed in the mild paranoid states in which the paranoid ideas are present but seldom verbalized or expressed overtly. It is only through the use of diligent and subtle analysis that these ideas are brought to the surface. When they have been exposed, we are sometimes surprised to see how deep-rooted and tenacious the idea-complex has been, and the extent to which it has implicitly affected the behavior of the individual.

Insulin and metrazol therapy in the history of mental diseases is brief. Insulin was isolated in 1922 and in 1923, Cowie, Par-

sons, and Raphael (41) used the drug with depressed patients but failed to notice any changes of personality structure. Isolated personality changes were observed in 1926 by Targowla and Lamache (42) and by Puca (43) in 1927, but like Miskolczy (44), Becker (45), Hoack (46), and Gründler (47), the insulin was used primarily as a means for improving the nutrition of their patients. It remained for Paul Schmidt (48), in 1928, to deliberately use insulin for its therapeutic effect. Jaschke (49), experimenting in 1929, noticed mental improvement in his patients although, like many others, he was still interested in the problem of nutrition. Other investigators such as Rosenthal and Beyer (50), Gallinek (51), Roberti (52), Appel, Farr, and Marshall (53), Stanesco (54), Slotopolsky (55), Gründler (56), Kuppers and Strehl (57), Steck (58), Mebel (59), and Jacob and Dousinet (60), had used insulin prior to the appearance of Sakel's monograph which introduced a new era into the treatment of the functional psychoses. Prior to Sakel, it was the universal procedure to administer carbohydrates along with the insulin as a precaution against the hypoglycemia. At the present time, a procedure similar to the following is widely used.

Patients selected for insulin therapy are treated three to six days each week. At approximately 6:30 A. M. on each treatment day, the patient receives an intra-muscular injection of insulin, beginning with 25 units on the first day. On successive days, the dose is increased in steps of 5-20 units until eventually a dosage is reached which produces a deep coma. This is the "initial shock dose" and ranges between 50 and 250 units. The dose is managed so that the patient becomes comatose in $2\frac{1}{2}$ -3 hours after injection. After allowing the coma to continue for approximately two hours, it is terminated by the administration of a sugar solution. Before the initial shock dose is reached, the sugar is given by mouth, while a nasal tube is used when the patient is in coma. In difficult arousals, or to off-set threatened convulsions, intra-venous injections are resorted to. During the course of insulin therapy, the appetites of the patients are ravenous and there is usually a gain in body weight of from 10 to 30 pounds. The first signs of

psychological improvement may be expected within the first three or four weeks.

It is important to note that while the efficacy of pharmacological shock therapy is generally accepted, there are those who oppose the method. Postle (26) insists that to accept shock therapy as near specific for schizophrenia would mean discarding psychobiological concepts. She points out that modern shock techniques strongly suggest older methods of shock treatment and doubts whether the method is justified. Baker (1), taking a less categorical position, concludes that repeated insulin shock produces a depression of cerebral function and, in some cases, an irreversible degeneration of the brain tissue, resulting in prolonged or permanent functional damage.

The selection of patients for pharmacological shock therapy is an important factor in the remission rate. It is now generally recognized that relatively young individuals in the early stages of a psychosis are more likely to profit by insulin and metrazol than is any other group. Vander Veer and Reese (36) believe that the best response is obtained in hebephrenic and paranoid cases which have had an acute onset, with marked symptomatology and hallucinations. Where the onset has been insidious, without hallucinations, the therapeutic prognosis is poor. These investigators indicate that the mixed psychoses respond poorly, although this view would seem to be in direct contradiction to that of Langfeldt (18), who finds it necessary to distinguish the typical schizophrenias from the atypical schizophreniform psychoses. In the latter group, which is the more amenable to shock therapy, are found psychopathic and manic-depressive symptoms.

The clinical phenomena usually associated with the hypoglycemic state may be described either in terms of autonomic manifestations, neurological signs, or the psychological and macroscopic behavioral picture. Autonomic symptoms include sweating, flushing, salivation, blanching, cyanosis, variations in pulse rate, increased pulse pressure and pupillary alterations, while the neurological signs include grasping-groping reflexes, a positive Babinski, changes in tonus, tics, myoclonic and choreiform jactations, generalized myoclonic movements, epileptiform

convulsions, extensor spasms suggesting decerebrate rigidity, and tonic neck reflexes. Ross and Malzberg (28) have described the physical and behavioral effects of insulin in terms of four syndromes corresponding to the observable progression of symptoms from the cortex to the medulla. In the cerebral syndrome, which includes the period up to two hours after injection, the picture is one of drowsiness, sleepiness, loss of contact, coma, defective orientation, and activation of the psychosis. The sub-cortical syndrome (2-3½ hours) is marked by myoclonic twitchings, clonic twitchings, primitive movements, primitive reflexes, choreic-athetoid movements, torsion spasms, hemibalismus, salivation and convulsive seizures. The mid-brain syndrome (3½-4½ hours) is characterized by convulsive seizures, tonic-tetanic spasms, dilated pupils, tonic torsion spasms, positive Babinski, Oppenheim, Rosso-limo and Hoffman reflexes, absent lid reflex and tonic laryngospasms. The medullary syndrome (4-5 hours) is marked by tonic extensor spasms, dilated pupils, rapid pulse, diminished reflexes, hypotonia, fixed pin-point pupils, slow pulse, pallor, depression of respiration and sluggish or absent corneal reflexes. Hyperkinetic symptoms are the most distinguishing feature of the sub-cortical syndrome while tonic-tetanic symptoms are most prominent in the mid-brain syndrome.

Despite an increasing number of studies on a biochemical level, little is known of the detailed mechanism of insulin action. It has long been recognized, however, that the essential feature of the action is the utilization of carbohydrates. Katzenelbogen and others (16) have demonstrated that, in addition to sugar, other blood constituents such as nitrogen, potassium, phosphorus, lactic acid and serum solids are altered during hypoglycemia. Studies have also been made of the changes in the fluid content of the brain tissue (12), of the changes in cerebral blood flow (13), and of the action of pharmacological shock on brain potentials (14).

The psychological picture of the patient undergoing insulin shock therapy shows euphoria, psychomotor unrest, changes in subjective body schema, visual hallucinations, and alterations in the perception of time, space, form and color. Sakel, Cameron,

Glueck, Schilder and others have observed that patients undergoing insulin therapy show an individual pattern of behavior which is repeated on successive treatment days. Wall (38), in studying these individual patterns of behavior, made careful observations of diffuse mass activity including biting and sucking movements, attempts to get out of bed, and similar acts indicative of a higher degree of neural integration. In terms of the analytic concepts of regression and catharsis, it might be possible to explain the prevalence of sucking and biting movements as regression to early oral stages. The general cathartic nature of the movements might be seen in the improvement which marks cases of this type. The anxiety apparent in these cases points to an underlying conflict situation.

Good (11) has shown that several types of behavior characterize the post-convulsive state. In the oral stage, the patients sometimes make an effort to retain the mouth gag, in addition to sucking and spitting movements. Anal behavior is apparent in fecal smearing and coprolagic activity, while genital manifestations are seen in the fingering of the genitalia, clawing of the vulva, exhibitionism, tugging at the penis and scrotum, and masturbation. Opposed to this analytic hypothesis is the fact that no progression through oral, anal, and genital stages is observed as the patients make a clinical improvement.

Young and others (61) have also noted certain significant psychological manifestations accompanying insulin therapy. In many instances the marked expansion of mood is observed immediately prior to the coma or during the awakening stages. At such times, the patient becomes jovial, laughs, makes facetious remarks, talks and shows other signs of the euphoric tendencies already mentioned. Other frequent patterns are those of depression and pre-occupation with ideas of death.

The use of metrazol (cardiazol, pentamethylenetetrazol) convulsive therapy was introduced in 1935 by Von Meduna (37). Metrazol, an analeptic drug which is quickly diffused and rapidly destroyed in the body, performs an irritative function in the manner suggested by Spiegel and Spiegel-Adolf (35), who have pointed out that epileptogenous agents such as metrazol may act

upon the nervous system by either of two mechanisms, or a combination of both. The production of a change in ion concentration on the surface of the nerve cell will alter the threshold level and when that level is reached, the convulsions take place. It is also possible that as a result of the diminution of the density of the cellular surface films, permeability is facilitated and the threshold is lowered.

Meduna originally advised an initial intravenous administration of 3cc. of metrazol with an increase of 1cc. if a convulsion is not obtained. A maximum of 10-12 cc. is recommended with 2-3 injections per week and a course of 20-30 convulsions.

Biochemical and physiological studies have indicated that between the time of the injection of metrazol and the convulsion, the blood sugar drops on an average of 10 mgs. per cent. During the convulsion the sugar level rises sharply and within one-half hour after the convulsion, rises to a point above the original level. The return to normal requires about two and one-half hours (21). Similarly, the hydrogen-ion concentration shows various values during the different stages of treatment. There is apparently a drop in the pH value after the convulsion (22), a period which also sees a marked rise (about 40 per cent) in the oxygen consumption rate leading to a generalized anoxemia. Maurer and other (23) have made a study of seven constituents in the blood of patients during the various stages of the metrazol convulsion. The investigation was made on the pH, lactic acid, glucose, carbon dioxide, phosphorus, calcium, and chloride, and revealed a marked acidosis during and immediately following the tonic convulsion. At these times, the pH rises to a point which is usually as high as is compatible with life. At the same time, there is a profound shift in certain electrolytes followed by a phase of recovery in which there is a return to chemical equilibrium.

Friedman (9) has maintained that there are certain metabolic and chemical changes in the brains of schizophrenics and, as a result of these processes, a functional barrier is set up against which metrazol acts through the medium of central nervous system irritations. That such irritation takes place, is evidenced by

the psycho-motor upheavals, toxic deliriform states, and convulsive reactions. Ziskind and Somerfield-Ziskind (62) believe that the injury to the brain resulting from the anoxia seems to destroy recently acquired impressions first, leaving earlier patterns intact. They suggest that recently acquired psychoses may be destroyed in the same way. Moore and Dean (24) advance a similar theory to the effect that the cerebral anoxia produces a destructive effect upon certain ganglion cells and, within therapeutic limits, this leads to a selective alteration of comparatively recent psychotic association pathways, leaving the older, more stable, pre-psychotic patterns relatively intact. Redlich (27) concludes that metrazol, like sodium amytal, acts on the neural centers of the brain stem, while Farrell and Vassof (8) suggest that some of the mental improvement may be explained by stimulation of the sympathetic nervous system. However, after studying the oxygen, carbon dioxide, and sugar content of arterial blood and internal jugular venous blood, Loman and others (20) have concluded that the changes in mental states following metrazol convulsions and insulin hypoglycemia cannot be explained on the basis of any common modification in either cerebral chemistry or cerebral blood flow. While this study does not preclude the possibility of other significant common physical factors, the existence of such factors have yet to be demonstrated. In the light of the fact that insulin and metrazol act differently but achieve similar results, it is necessary to find a common denominator which Sagebiel (29) believes he has found in an "artificial controlled death threat directed to the primitive centers of the self-preservative instinct in the brain stem, which demands withdrawal of the libido from its somatic or psychic investments."

The behavioral changes following the metrazol-convulsion have been aptly described by Schilder (33), who studied phenomenon observed in the half-hour immediately following the convulsion. He found that patients exhibited a marked difficulty in remembering the names of objects, with a tendency to perseveration and to paraphasia. He noted, also, difficulty in copying Gestalt patterns of the type devised by Bender (2), with a reversion to primitive and archaic forms, pointing to a profound disturbance

of the form-function. Cohen (3), working in the affective sphere, determined that crude sensation appears first, followed by coarse sensory discrimination. The ability to focus attention on objects precedes the ability to use them correctly. Memory is last to return, with a progressive delimitation of amnesia. There are apparently two types of mental defect produced by the shock therapy; the first is a retrograde amnesia in immediate association with the convulsion and might be compared to the amnesia associated with idiopathic epilepsy. The second type of memory defect is a persisting memory disturbance which may remain active for a number of weeks.

Cohen (4) has spoken of the fear experiences during the pre-paroxysmal phase of convulsive therapy. In one experiment (5), a "fear state" was induced by giving slow intravenous injections of minimal amounts of metrazol. In this way, fear states of from a few minutes to several hours were observed. This group of patients was compared with another group which received convulsive doses of metrazol. It was found that the procedure with "induced fear" was of less therapeutic value than the convulsions. It was found also that the "fear of treatment" seen in some patients was of little or no therapeutic significance. Similarly, Cook (7), who carefully studied the degree of fear manifested in 275 cases of cardiazol and triazol therapy, correlated the fear states with the result of treatment and concluded that there is no evidence for assuming that fear exerts any curative influence. Lipschutz and others (19) have also noted the fear of impending danger. However, they emphasize that the general therapeutic setting of pre-coma and emergence from coma produces an attitude of "infantile dependence." It is possible to utilize this attitude of dependence and trust as a focal point for the extension of responsibility to the patient. The danger, of course, lies in the fact that the patient may find the state so satisfactory that he will reject the responsibility and remain fixated at this level of dependence. These authors also point out that the reactivation of psychotic symptomatology may have some cathartic value.

From the point of view of more general behavior, Winkelman (39) has pointed out that the metrazol convulsion is fre-

quently preceded by a short cough and generalized body twitching. The convulsion itself is a generalized seizure followed by intense rigidity and a slow, but powerful, closing of the mouth. Cyanosis, dyspnea and apnea are observed and, after a few seconds of cessation of breathing, the patient inspires and relaxes. Cohen (6) describes the grand mal seizure as one involving successively higher levels of the nervous system in which the spasms seem to occur simultaneously, in all muscles, in a bilateral symmetrical manner. Sagebiel (30) has noted certain behavior in metrazol patients, including tetany due to hyperventilation, biting and bruising of the lips and tongue, severe motor overactivity and anxiety following sub-convulsive doses of the drug. Following the convulsion, patients are frequently in a state of confusion for a variable length of time ranging up to two hours or more. It appears that convulsive therapy operates on neither a particular mental disease nor group of symptoms but it influences a number of morbid reaction types. It is apparently less important what the disease is than how deep-rooted it is. Paraphrenic states without affective or conduct change are not much influenced while the stuporous states are more easily eradicated than hebephrenia and praecox simplex. Recurrent conditions seem to respond well although habituated delusional systems which have little affective tone are not readily modified.

From the point of view of the present investigation, the most significant question is that related to the psychological changes which are observed during the course of pharmacological shock therapy. While we have seen that the biochemical and physiological mechanisms involved in insulin shock and metrazol convulsion are essentially different, we find that the behavioral modifications resulting from each type of therapy parallel one another with a high degree of consistency. It becomes desirable, at this point, to consider some of the theories which have been advanced to explain this equivalence of behavior. Sakel (32) has suggested that "the hypoglycemia state weakens, inhibits, and finally represses that portion of the mind which happens to be most active at the time, so that hitherto latent subdued and repressed elements are again brought to the surface so that they

can prevail again over those which are now repressed. In cases which run a favorable course, the repeated hypoglycemic states finally eliminate the psychosis so that normal personality can again achieve complete dominance." This hypothesis, despite its obvious weakness, is widely accepted, in one form or another, by many investigators. Moore and Dean (25) adhere to a similar position when they state that the cerebral anoxia induced by the shock therapy destroys certain ganglion cells and, within therapeutic limits, this destruction leads to a "selective alteration of comparatively recent psychotic association pathways, leaving the older, more stable pre-psychotic patterns relatively intact."

More suggestive, albeit more highly symbolic, are the attempts made to explain the psychological effect of pharmacological shock therapy in terms of various psychoanalytical constructions. Schilder (34) views the metrazol convulsion, like the epileptic fit, as permitting the patient to experience phenomenological death and rebirth. He states:

The individual's psychic energy is free once more again. The previous fixations of the libido have lost their importance and there is renewed interest in the persons next to the patient. The victory over death threat, expressed in the epileptic fit and lingering on in the perceptual and aphasic difficulty, enables the individual to start life and relations to human beings all over again. The previous fixations of libido, lying in a more personal layer of experience, are washed away by the recovery from a cataclysmic catastrophe in the depths of the organism.

In this hypothesis, Schilder is in essential agreement with Jelliffe (15), who sees in the behavior of the patient a regression to primitive, infantile levels of the intrauterine type. In addition Jelliffe recognizes a death threat to the organism as a result of the withdrawal of glycogen forces. He has expressed his position in the following words:

The death threat, by the withdrawal of glycogen, forces a definite withdrawal of libido from the aggressive hostile anal, oral and other negativistic behavior patterns. The coma, to say nothing of its antecedent less regressive phases, brings the individual practically into an intrauterine bath of primary narcissitic omnipotence.

By the hypoglycemic threat, we can envisage a type of phylogenetic dissection by a metabolic tool from frontal forebrain back to the medullary,

respiratory and vagus nuclei and a decerebration experiment of a very subtle form by a pharmaco-dynamic type of instrumentation.

The hypoglycemic death threat is unique. Genetically considered, it may be thought of as a very primordial, primitive and massive type of threat which strikes at the very initial stages of life's upholdings. When it is recalled that even before primitive life appeared on earth, that primitive carbohydrates were formed by the action of ultraviolet light on CO_2 , H_2O and NH_3 , it requires little paleochemical insight to realize that carbohydrates were among the first energy transforming substances which created life. The emphasis upon carbohydrate utilization in muscle-nerve functioning has a long phyletic history, hence the death threat is a much more vital one coming from this direction than from almost any other.

Glueck and Ackerman (10), in describing schizophrenia as a distorted personality structure which represents an equilibrium between ego-destructive tensions arising in the frustration of instinct and the ego-constructive efforts of the organism, point out that metrazol therapy invokes anxiety, even panic, and the patient seeks the aid of others by establishing plastic emotional attitudes. The personality is profoundly threatened and is forced back to more primitive, biological levels of undifferentiation and, as a result, a more normal differentiation is made possible. The loss of stability afforded by the psychosis necessitates a re-synthesis of the ego structure. Glueck and Ackerman have said:

We witness an early provocation of great anxiety; often the amount of anxiety so elicited, mounts rapidly to a point where the patient enters a state of panic. In close association with this, the patient becomes distinctly more communicative, he expresses his anxiety freely and verbalizes his paranoid fears. The intensity of his fears is so great that he becomes not only able, but extremely anxious to solicit help and the protection of the people surrounding him against the dire threats to his life which he has conjured up in his phantasy. His contacts with people, therefore, become more pliable.

The patient's delusions become aborized, diffuse, chaotic, and unstable. It would appear as though the treatment procedure induces a state of panic, and unstable disorganization of the patient's personality.

One might assume that the patient is forced out of a chronic fixed adjustment into an acute schizophrenia disorganization somewhat on a parallel with the condition which ushered in the original illness.

We are disposed to agree with the opinion of schizophrenia which conceives it as a destructive process which is never quite carried to its

logical terminating point, namely, complete destruction, or in terms of the psyche, total death. In this sense the process of schizophrenia is always a partial one; side by side with the process of ego dissolution there is a concurrent movement laboring toward preservation of the normal ego relations.

Metrazol treatment is a radical disturbance of this equilibrium and is carrying the process of destruction on a profound physiological plane almost to death, but not quite. Perhaps it is not too far-fetched to speculate that the tremendous artificial propulsion toward the destruction of the ego which comes in the wake of a metrazol convulsion, brings with it an intensification of the opposite tendency toward normal restoration of the ego structure.

While these theories are extremely provocative, one must not lose sight of the fact that none of them are as yet based upon experimental evidence. At best, they are inferences based upon clinical observations in uncontrolled situations. The present writers have been unable to find in the literature any attempt to describe the psychological effects of pharmacological shock therapy in terms of established theoretical systems. However, such an approach, adequately developed, would aid the clinician in understanding the behavioral reorganization from the patient's viewpoint. Such an understanding would facilitate the selection of the most desirable psychological treatment to be administered with the medical therapy. The following important questions might be considered in the light of Gestalt dynamics. Why, after the shock, does the patient's world *sometimes* reorganize into a somewhat different system than that which existed before the disturbance? Why, at other times, does the behavioral system reorganize into its original schizoid condition? What form of psychotherapy might best be utilized, during the arousal from the coma, in bringing about the desired reorganization?

Many of the hypotheses presented earlier in the paper contain valuable suggestions for the construction of such an organismic hypothesis. The remainder of the article constitutes a tentative Gestalt picture of the psychological changes resulting from shock therapy.

In accordance with Köhler (17), the isomorphic counterpart of the reorganization of the behavioral field of the patient must

be a reorganization of the force systems of his brain field. In the reflex loop concept presented by Williams (40), the function of the sensory system is viewed as that of a disturber of the brain field. Moderate sensory activity results in a mild, temporary behavioral (and brain) disturbance, followed by a quick reorganization process. Excessive sensory activity results in a severe behavioral disorganization and partial disintegration of the behavioral field, with a consequent reorganization which may be quite different from the original organization, the degree of this difference being a function of the nature of the prevailing constraining conditions.

It has been pointed out that ionization changes of the neuron cell surface result from the use of metrazol in shock therapy (35). An increased polarization in this area lowers the threshold, or increases the sensitivity. The additional factor of decreasing the density of neural surface film, which increases the permeability of the neural membrane, further decreases the threshold. The resulting excessive sensory activity, in addition to the provoking of muscular spasms, causes a disintegration of existing behavior organizations responsible for the schizophreniform personality.

Friedman (9) found chemical changes in the brain of schizophrenics which may function as a barrier. *It is quite likely that this condition is the isomorphic counterpart of the characteristic psychological barrier which has been built up between the behavioral field of the schizophrenic patient and his geographical environment.* The excessive sensory activity instigated by metrazol breaks through and partly disintegrates this barrier, with a corresponding disintegration of the psychological barrier.

Once these effects have broken through the barrier they are able to disturb and disintegrate the behavioral field. Symptoms of this behavior disintegration, this change from an articulated heterogeneous field containing behavioral "things" to a completely homogeneous field, have been described by Schilder (33) and Jelliffe (15). Schilder noted that patients under treatment lost their ability accurately to reproduce Gestalt configurations. In their behavioral field, shapes and borders tended to disintegrate. Figures, or behavioral things, were losing their identity; they were

merging into the background from which they had earlier organized. Jelliffe reported that patients regress to infantile stages—characterized by poor differentiation and mental fogginess.

Because certain geographical constraints existed sometimes during the behavioral development of the patient which ran counter to the dynamic direction of the behavioral field, the behavioral field built a psychological border which shielded itself from these constraining conditions, and thus articulated out of step with environmental conditions. The shock therapy disintegrates this "out of step" behavioral field. The field reverts to its original primitive state of fogginess and only a remnant remains of previous behavioral objects. The partially disintegrated ego remains in the phenomenal environment which serves as the starting point for mental development.

This undoing of the field, which terminates in coma, characterizes the early stages of the therapy. As the effect of the drug diminishes and the coma wears off, the ego finds itself in a homogeneous environment which is beginning to articulate.

During this regrowth of the mind two opposing sets of forces are at work which determine the extent of the "cure." These critical sets of forces are described in the following paragraphs.

(a) According to Schilder (34), the destruction of the behavioral environment is not quite complete; phenomenal death is approached but is not completely experienced. This means that there still exist rudiments or vestiges of the previous phenomenal objects and their ego relationships; some of the framework remains. Just as the rebuilders of a city after a severe earthquake or bombing raid tend to reconstruct in accordance with partially remaining frameworks, so the behavioral reconstruction is guided in its directionality by the remaining partial behavioral structures. These guiding forces hinder a cure in that the reconstructed behavior field follows the pattern of the old one which was that of the schizophrenic condition. The old behavioral barrier, the "wall around the city," may be reconstructed.

(b) Glueck and Ackerman (10) have reported the panic of patients during the treatment. This is perhaps due to the fact that the ego, like any other system, has the directionality to pre-

serve itself, to continue its existence. This nearness to phenomenal death, to a complete disintegration of the ego, forces the ego to form stabilizing relationships with any developing behavioral object. This includes those phenomenal objects which have geographical counterparts, such as other people. The schizophrenic condition involves the ignoring by the ego of any perceptual object having a geographical counterpart. This closeness to complete ego disintegration forces the ego to drop this ignoring attitude and to grasp any behavioral object which may lend its support. The development of these new ego relationships *is* the cure.

The psychotherapeutic implication which should follow from this hypothesis would be for the clinician, serving as a behavioral object to the patient, to establish rapport immediately following the coma and during the early stages of the behavioral rearticulation. *The medical treatment alone is not sufficient; it merely affords the proper psychological setting for the development of the desired ego relationship.*

REFERENCES

1. BAKER, A. B. Cerebral damage in hypoglycemia. *Amer. J. Psychiat.*, 1939, 96, 109-127.
2. BENDER, L. Gestalt function in visual motor patterns in organic disease of the brain. *Arch. Psychiat. & Neur.*, 1935, 33, 300-329.
3. COHEN, L. H. The therapeutic significance of fear in the metrazol treatment of schizophrenia. *Amer. J. Psychiat.*, 1939, 96, 1349-1357.
4. See Reference 48. Also, *Arch. Psychiat. & Neur.*, 1939, 41, 489.
5. See Reference 48.
6. COHEN, L. H. Metrazol treatment of schizophrenia. *J. Nerv. & Ment. Dis.*, 1938, 91, 502.
7. COOK, L. C. Fear and convulsion therapy. *J. Ment. Sci.*, 1940, 86, 484-490.
8. FARRELL, M. J., & VASSOF, E. Effect of insulin shock on the heart and blood pressure in treatment of schizophrenia. *Arch. Psychiat. & Neur.*, 1940, 43, 784-791.
9. FRIEDMAN, E. The irritative treatment of schizophrenia. *Amer. J. Psychiat.*, 1937, 94, 355-372.
10. GLUECK, B., & ACKERMAN, N. W. The reactions and behavior of schizophrenic patients treated with metrazol and camphor. *J. Nerv. & Ment. Dis.*, 1939, 90, 310.
11. GOOD, R. Some observations on the psychological aspects of cardiazol therapy. *J. Ment. Sci.*, 1940, 86, 491-501.
12. HALL, G. E. Physiological studies in experimental insulin and metrazol shock. *Amer. J. Psychiat.*, 1939, 95, 553-566.
13. See 12.
14. See 12.
15. JELLIFFE, S. E. Discussion of B. Glueck's paper, "Clinical Experiences with

- the Hypoglycemic Therapy of the Psychoses" New York Neur Soc, Jan. 12, 1937. *J. Nerv. & Ment Dis.*, 1937, 85, 575-578.
16. KATZENENBOGEN, S., & HARMS, H. E. The insulin treatment of schizophrenic patients. *Amer. J. Psychiat.*, 1939, 95, 793-797.
 17. KOHLER, W. The Place of Value in a World of Facts New York: Liveright, 1938.
 18. LANGFELDT, G. Duration of spontaneous remissions of schizophrenic psychoses: What can be attained with the shock therapy? *Arch. Psychiat. & Neurol.*, 1939, 42, 543.
 - 19.. LIPSCHUTZ, L. S., CAVELL, R. W., LEISER, R., HINKO, E. N., & RUSKIN, S. H. Evaluation of therapeutic factors in pharmacologic shock. *Amer. J. Psychiat.*, 1939, 96, 348-360.
 20. LOMAN, J., RINKEL, M., & MYERSON, A. Metrazol convulsions *Arch. Psychiat. & Neurol.*, 1940, 43, 682-692.
 - 21—23 MAURER, S., WILES, H. O., & MARBERG, C. M. Blood chemical changes occurring in the treatment of psychogenic mental disorders by metrazol convulsions. *Amer. J. Psychiat.*, 1938, 94, 1355-1362.
 - 24.—25. MOORE, C. D., & DEAN, S. R. Results and theory of action of metrazol therapy. *J. Conn. State Med. Soc.*, 1939, 8, 338
 26. POSTLE, B. Shock therapy in the neuroses. *Ohio State Med. J.*, 1939, 35, 385.
 27. REDLICH, F. C. Metrazol shock therapy. *Amer. J. Psychiat.*, 1939, 96, 193.
 28. ROSS, J. R., & MALZBERG, B. A review of the results of pharmacological shock therapy and the metrazol convulsive therapy in New York State. *Amer. J. Psychiat.*, 1940, 96, 297-316
 - 29.—30. SAGEBIEL, J. Shock therapy in the neuroses. *Ohio State Med. J.*, 1939, 35, 385.
 31. SAKEL, M. Neue Behandlungsart Schizophreniker und verwirrten Erregter *Wien. klin. Woch.*, 1933, 46, 1372.
 32. ———. The origin and nature of the hypoglycemic therapy of the psychoses. *New York Neur. Soc.*, Jan. 12, 1937 (Proceedings).
 33. SCHILDER, P. Notes on the psychology of metrazol treatment of schizophrenia. *J. Nerv. & Ment. Dis.*, 1939, 89, 133-144
 34. ———. (Discussion: See Reference 15.)
 35. SPIEGEL, E. A., & SPIEGEL—ADOLF, M. Physio-chemical mechanisms in convulsive reactivity. *J. Nerv. & Ment. Dis.*, 1939, 90, 188-290.
 36. VANDER VEER, A. H. & REESE, H. H. Treatment of schizophrenia with insulin shock. *Amer. J. Psychiat.*, 1938, 95, 271-290.
 37. VON MEDUNA, L. Die Konvulsionstherapie der Schizophrenia. Halle: Marhold, 1937
 38. WALL, C. Observations on the behavior of schizophrenic patients undergoing insulin shock therapy. *J. Nerv. & Ment. Dis.*, 1938, 91, 1-8.
 39. WINKELMAN, N. W. Metrazol treatment in schizophrenia. *Amer. J. Psychiat.*, 1938, 95, 303-315.
 40. WILLIAMS, R. D. Studies in contemporary psychological theory: II. What is Gestalt psychology? *J. of Psychol.*, 1938, 6, 99-114
 - 41.—60. WORTIS, J. Sakel's hypoglycemic insulin treatment of psychoses: History and present status. *J. Nerv. & Ment. Dis.*, 1937, 85, 581-595.
 61. YOUNG, G. A., YOUNG, R. H., & ROUCEK, L. Experiences with the hypoglycemic shock treatment of schizophrenia. *Amer. J. Psychiat.*, 1937, 94, 159-170.
 62. ZISKIND, E., & SOMERFELD-ZISKIND, E. Loss of recently acquired learning due to metrazol therapy. *Bull. Los Angeles Neur. Soc.*, 1939, 4, 77.

TREATMENT OF A CASE OF ANXIETY HYSTERIA BY AN HYPNOTIC TECHNIQUE EMPLOYING PSYCHOANALYTIC PRINCIPLES

MARGARET BRENNAN AND MERTON M. GILL

The Menninger Clinic

Hypnosis is not very widely used in present-day psychiatric practice, though sporadic reports of remarkable "cures" appear periodically in the literature. With increasing frequency, suggestions are heard that hypnosis needs to be re-investigated and re-evaluated in the light of the remarkable advances in psychodynamics that have taken place in the last half century. That striking psychological phenomena can be evoked through hypnosis is unquestioned. It would seem then that in some way this powerful tool can be developed into an effective psychotherapeutic method.

Hypnosis was first used as a technique for alleviating symptoms by simply suggesting that they would disappear. The use of hypnosis for purposes of direct suggestion suffers all the usual disadvantages of a technique of symptomatic therapy, as contrasted with one aimed at the underlying etiology. Recurrences of the illness either in the original form or in a new one are very frequent.

It was Janet, Breuer and Freud who carried hypnosis from its use as a tool of suggestion to its employment as an aid in so-called cathartic hypnosis. One of Freud's famous early formulations was that "the hysteric suffers from reminiscences" and he utilized the hyperamnesia of hypnosis to recapture the traumatic memories. The re-living of memories in the hypnotic state with

the full expression of their intense associated emotions, Freud called "abreaction." He abandoned hypnosis because he found many patients whom he could not hypnotize, because some patients cured by abreaction disconcertingly relapsed and perhaps most significantly because he felt that he could have obtained the same memories by his technique of free association. It was by way of this technique that he went on to make his further epochal psychodynamic discoveries and to formulate the principles of psychoanalytic therapy. This paper is the presentation of a treatment method which combines the technique of hypnosis with psychoanalytic methods. Our work is part of a research program in the re-evaluation of hypnosis.

The patient with whom we worked was a conventional middle-class housewife in her middle thirties who was referred to the Menninger Clinic by an internist who had been unable to find any organic basis for her symptoms. She had been ill periodically for seven years, continuously for two years and with marked exacerbation for the preceding six months. The symptoms which concerned her most were daily nausea and vomiting, severe lower abdominal pain, anxiety and depression and trembling of the left hand. There were numerous other symptoms, including headaches, dysmenorrhea, a fear of falling, palpitation and nightmares in which she saw herself being killed or buried.

She gave her history with the vagueness, the many gaps and the placing of the onset of symptoms by successive stages in the more distant past which Freud has described as so characteristic of hysteria.

Although quite intelligent, the patient had a somewhat child-like naiveté. For example, she engaged other patients in the waiting room in conversation about her symptoms and once queried a stranger just outside the Clinic grounds about the work of the Clinic, saying she wanted to be sure that her money was being well spent. She said she supposed this person was another patient who could give her the benefit of his experience.

The history and psychiatric examination pointed unmistakably to a diagnosis of anxiety hysteria. Psychological test findings supported this diagnosis and though she attained an I. Q. of only

100, it appeared that her effective intelligence was diminished by her great anxiety and preoccupation with fantasy.

Psychoanalysis was impracticable in this case for the same reason that it is so often impracticable. The patient lived 200 miles away, her husband and two children needed her. She was in poor circumstances. She was very eager for help, however, and it was decided to determine whether she would be suitable as a case in the hypnosis research project.

The patient's degree of hypnotizability was determined first. She proved to be an excellent subject and went into a deep trance in the first session. She was permitted to talk at will and began at once to recount a nightmare that she had had the night before, of the sort which constituted one of her presenting complaints. This was the beginning of a course of treatment in which the patient was seen every day for an hour to an hour and a half, six days a week for sixty-seven interviews. At first the interviews were conducted chiefly by the author who was experienced in hypnosis (M. B.), later by the author who had worked with the case originally as a psychiatrist (M. G.). Both authors were always present. The interview was always begun with a short session in the hypnotist's office, in which the patient usually described how she had felt during the last twenty-four hours, frequently summarized what she had learned in the previous day's hypnotic session and occasionally advanced new ideas and formulations that had occurred to her since that session. If the patient had dreamed the night before, she usually related this dream in the preliminary interview. The patient was then taken into another room and hypnotized. After the first few hypnotic sessions she was very easily placed in hypnosis simply by the therapist's counting to ten. After the hypnotic session, a few words were occasionally exchanged about how the patient was feeling, she sometimes made some comments upon the material of the hypnotic hour and the appointment was made for the following day. After the first hypnotic session, the patient remarked that to her surprise she was able to remember everything that had gone on in hypnosis. She was reminded that nothing to the contrary had been suggested and thereafter she continued to remember all the material

of the hypnotic sessions except in a few instances where amnesia was specifically suggested and in a number of others where the forgotten material proved to have special dynamic significance.

The technique which was followed in the hypnotic sessions was in general one of directed associations. Usually the hour was begun by taking up the topic, which from her pre-hypnotic remarks, appeared to be uppermost in the patient's mind. This was discussed with her until some question was reached which she was unable to answer or to which her answer was unenlightening to the therapists. The formulation which was then most generally applied was "I will count to a certain number and when I reach that number you will tell me the first thing that occurs to you in connection with so-and-so." Variations of this formula were also used. Sometimes the patient was told that a single word would occur, sometimes a picture. When it was particularly difficult to elicit material, she would sometimes be told that a number would occur to her which would indicate the number of letters in a word, and then these letters would be obtained in jumbled order. For example, she was once told that at the count of five, a number would occur to her which would be the number of letters in a word, which would help to answer the question under consideration. The number turned out to be four. The therapist then counted to five to get a letter, which was "m". The other letters obtained similarly were "o", "b", "w", and the four as re-arranged by the patient spelled "womb". It would frequently take the patient some little time to recognize the word which was constituted by a jumble of letters, even when it was obvious to the therapists. Frequently we would ask the patient to associate to a certain idea by producing an earlier memory, which had the same emotional connotations as the idea. These same types of associative techniques were used in the exploration of dreams. We found that the dreams as given in the hypnotic state, in contrast to the pre-hypnotic interview, were much more replete with significant detail through which their meaning could be most quickly learned.

A few specialized hypnotic techniques were also used. On a

number of occasions, the patient was told that she would have a dream in connection with a certain problem and this was almost always successful. On several occasions, the patient was made in hypnosis to complete an incomplete dream action. In one dream, for example, she searched through an entire house, though she didn't know what she was looking for. The last place in which she was going to look was the basement. But as she began to go down the stairs she awoke in terror. In the hypnotic hour she was made to go down into the basement and describe what she found. On one occasion the technique of induced regression to an earlier age was used with rather striking results. The patient was regressed to the age of five. We asked her where babies came from. She told us that babies were vomited up but refused to tell her theory as to how they were made. That night she had a dream which was a repetition of an experience at the age of five, in which she had been told of fellatio by a little girl friend. She then told us that it was this memory that she had refused to communicate while she was regressed.

The interviews were conducted with a great deal more activity on the part of the therapist than is the case in the usual psychoanalytic interview. The hypnotic interviews at times approximated question and answer interviews, rather than prolonged, uninterrupted, spontaneous verbalization on the part of the patient. The therapists followed whatever lead appeared to be the most promising and if after five or ten minutes it seemed to be yielding nothing, it was abandoned for another topic. Nevertheless, interpretations, except as will later be described with reference to transference phenomena, were kept at a minimum. Most interpretations were merely elaborations of the patient's spontaneous insight. Although it was the therapists' tentative formulations which led down to open certain topics, other psychiatrists who have read the verbatim records of the sessions have agreed that almost always the patient's formulations were spontaneous. This spontaneity was further attested to by the fact that much of the patient's production was unanticipated. The material unfolded with dramatic rapidity and withal in a well-ordered sequence. Most hours ended with unresolved problems

and the patient frequently began the next hour by plunging into the previous day's problem as though the intervening 24 hours were blotted out.

Her freedom of behavior in hypnosis was in vivid contrast to that of the ordinary hypnotic subject as we usually see her in the experimental situation. At first she sat immobile, just as a subject usually does. But one day she moved her arm in describing something. She was startled to find that she had moved it, but when it was pointed out that she had not been prohibited from moving, her behavior became more and more free until soon there were hours when she was pounding on the arms of her chair in furious anger, and others in which she wept in despair, all the while in deep hypnosis. This illustrates what we believe to be one of the most important aspects of the treatment. Hypnosis can take place in a permissive atmosphere in which the patient can be given wide latitude of expression and behavior. Hypnosis can be divested of its time honored implications of the subject's absolute subservience, and immobility, and helplessness to act except under the initiation of the all-powerful hypnotizer.

When we were attempting to explore more adequately a memory that the patient had recalled, we would frequently suggest that the recall would be as vivid as though she were again experiencing the original event. The actions with which the patient accompanied some of her verbalizations attested to the vividness with which she was re-living experiences. When she recalled in hypnosis how she had in a dream searched for something in the sky, she raised her head and moved it as though searching on high, although her eyes remained closed. When she was asked to look into an open grave, which in the dream she had been afraid to do, she craned her head forward with such genuine apprehension and expectation that the effect was quite eerie. She was unaware of such bodily movements and expressed amazement when they were called to her attention. In her verbalization she would switch back and forth in a remarkable way from past to present tense, as though she was at one moment oriented in the present and at the next moment vividly re-living the past. We feel that this intricate interweaving of the past and present

represented the re-integration of the repressed past into her conscious ego.

Some of the patient's symptoms disappeared in the manner which Freud described as occurring in cathartic abreaction and it was in connection with these that the experiences of re-living were most vivid. A notable example was her fear of falling which disappeared soon after the beginning of the treatment, immediately after her recall and re-living in hypnosis with intense affect two childhood falling experiences. In one she fell from a high chair and in the second, she fell from a hammock at the age of seven. It is difficult to describe the vividness with which she cried in terror, "Save me, Dr. B., save me—I'm falling!" That this re-living in hypnosis is not simply a re-living of the original experience but rather a re-living which takes place in the frame of the present personality structure is clearly shown by her calling on the doctor to save her—and indeed it is likely that it is this very difference which permits the resolution of the conversion mechanisms.

Other symptoms disappeared only when their symptomatic meaning had become clear to the patient. The hand-trembling, for example, was not completely relieved until the very end of the treatment when the patient developed what seemed to us to be full insight into its symbolic meaning.

The case material and dynamics which were actually obtained we do not report here. We plan to publish them in detail, but believe that it is more appropriate here to discuss the method rather than the individual case. In brief, it can be said that we obtained the kind of material which is familiar to all psychoanalysts and differs in no significant way from that of hysteria. We learned, for example, that the patient's vomiting represented *both* a rejection of impregnation and a fantasy of delivery, *both* of which she unconsciously thought took place by mouth. Material substantiating the psychoanalytic discoveries concerning infantile psychosexual development appeared in unusually clear and abundant detail, not simply in terms of reconstruction on the basis of fantasies, but rather by way of direct recall of childhood memories and ideas. By the end of the treatment the patient

had been entirely relieved of her symptoms, not only those of somatic conversion, but also the psychological disturbances, such as her anxiety and depression. Shortly after she returned home her youngest child became seriously ill, and although some of her anxiety returned, she weathered the storm very much better than she had previous similar episodes. This experience, however, has led her to feel that she needs a few more weeks of treatment and she plans to return.

We call the treatment we used "hypoanalysis" because we believe it combines significant features of hypnosis and psychoanalysis. We should like to discuss how the present method resembles and how it differs from each of these.

One of the basic differences between this treatment and hypnosis as previously used and one of its essential similarities to psychoanalysis is the handling and the use of the transference. Undoubtedly the most significant addition to the technique of psychoanalysis that Freud made after abandoning the hypnotic method in favor of free association, was the recognition and the interpretation of the transference. It is now well recognized that the transference phenomenon exists in every psychotherapeutic situation—indeed in every relation between physician and patient and in many other life situations. The unique contribution of psychoanalysis is the awareness of and interpretation to the patient of this transference. The patient's recognition of his enactment in the psychotherapeutic situation of his previously formed behavior patterns is one of the most important vehicles by which he obtains insight.

Psychoanalysis is no longer the only method which makes conscious use of transference. An attempt has been made by a few psychiatrists to carry over the technique into general psychotherapy and many psychoanalysts believe that when the unconscious mechanisms of the patient-therapist relation are thoroughly understood by the therapist, any method of psychotherapy can be made more effective.

Although the dynamics of the induction of hypnosis have only begun to be explored, there seems little doubt that there is an intimate relation between the induction of hypnosis and the

transference between hypnotizer and subject. This is not to deny that other factors may be involved too. Perhaps the first clear formulation of this point of view was made by Ferenczi, who differentiated between "mother" and "father" hypnosis. He believed that the force impelling the subject to accept hypnosis was in the first instance love, and in the second, fear. Ferenczi attributes cures through hypnosis to the persistence in the patient of this unconscious transference. In our patient, the case of hypnotizability varied with the state of the transference. When she was in an angry, rebellious mood, the hypnosis was somewhat more difficult to induce and was not as deep. When she was in a state of positive transference, she went quickly to sleep, with a deep sigh of unmistakably erotic significance.

We are here not as concerned with the significance of transference in the induction of hypnosis as with the interpretation of the transference as it arose within the hypnotic interviews. That is to say, we made no attempt to interpret the transference by which the patient became hypnotized, but we did interpret the transference which developed in the psychotherapeutic relationship in a manner very similar to that in a psychoanalysis. The patient's transference in this case ran the gamut from warm expressions of affection under the influence of which her symptoms were markedly alleviated simply by her entrance into the therapists' office, to wild outbursts of rage, anger and jealousy. The initial interpretations of the transference were an exception to the general rule that interpretations were not given by the therapist until spontaneously suggested by the patient. After these first interpretations, however, with explanations to the patient of the nature of the transference, just as she did into the progressive unfolding of the psychodynamics of her neurosis. The specific details of the transference could be given only in the light of the complete treatment history and therefore only a few examples will be given here: The patient vomited instantly that the psychiatrist's wife had just had a baby. She spontaneously recognized that this vomiting was based on the same jealousy and wish to have a baby by a father figure that she had felt when her siblings were born. Under the influence of an oral theory of birth she

had as a child reacted to their appearance by vomiting. There were several instances of unusual transference behavior arising from the fact that the patient was being treated by a man and a woman together. The most striking of these occurred early in the treatment when, at the end of an hour, the therapists left the room for a moment before terminating the hypnosis. When they returned to the room the patient had become severely nauseated and disturbed and explained that she had suddenly recalled an experience of over-hearing parental intercourse. The recall had evidently been stimulated by the absence of the two therapists. On another occasion, during the patient's induced regression to the age of five, one of the therapists whispered a remark to the other and the patient called out, "I hear Daddy and Mother whispering—they have too many secrets!"

There is a fundamental opposition between the handling of transference in earlier forms of hypnosis and hypnosis as we used it. In hypnosis, by direct suggestion the hypnotist strengthens and exploits this unconscious transference by investing himself with mysticism and an aura of the super-natural. That Freud's difficulties with cathartic hypnosis had much to do with the fact that he had not yet developed the technique of interpretation of transference can be clearly seen in the following remarks from his autobiography: "Increasing experience had also given rise to two grave doubts in my mind as to the use of hypnotism even as a means to catharsis. The first was, that even the most brilliant results were liable to be suddenly wiped away if my personal relation with the patient became disturbed . . . and one day, I had an experience which showed me in the crudest light what I had long suspected. One of my most acquiescent patients woke up on one occasion, and threw her arms around my neck." Freud is obviously speaking of negative and positive transference. Recent work of Erickson and Kubie reports the utilization of the transference relationship to achieve therapeutic results, but the transference itself is not analyzed. In the case which we report, the interpretation of the transference was, as in psychoanalysis, a vehicle by which we led the patient to insight into her neurosis.

The objection which is most frequently raised to the use of

hypnosis as a psychotherapeutic tool is the statement that the ego of the patient with its resistances and defenses is not involved. In fact, it is said that while this is the very reason that one has direct access to unconscious material, it also accounts for the fact that hypnotic cures are so frequently temporary. It is believed that the hypnotic technique obliterates the dynamic activity of the ego, that whatever insight is gained is not assimilated into the ego and that therapy therefore effects no significant change in the ego. We believe that this objection to hypnosis arises not because hypnosis in its very nature obliterates the ego, but because of the way it is used even in cathartic hypnosis. The attempt of the therapist in that method is simply to elicit the traumatic experiences under the theory that their re-living will cause their harmful effect to disappear by abreaction. No attempt is made to help the patients integrate newly gained insight into their egos. That the abdication of the ego is not a necessary condition of the hypnotic state, we believe has been thoroughly demonstrated by much recent hypnotic work. Our patient showed strong resistances, both in and out of the hypnotic sessions. At one point in the treatment, in a phase of strong negative transference and shortly before the emergence of some especially traumatic material, she was on the verge of disrupting the treatment to seek surgical relief for her complaints. Once when she was told in the hypnotic hour that she would have a dream the following night, she kept herself awake all night to prevent herself from dreaming. In the induced regression, when she was asked how babies are made, she replied "I won't tell you, even if you spank me," and indeed did not tell. On many other occasions she at first refused in hypnosis to communicate the material present in consciousness.

The types of formulation of insight that our patient made in the hypnotic state were no different from those in the prehypnotic interviews. In hypnosis she reacted to the revelations about herself with the same shame, chagrin and attempt at denial that she showed in the waking interviews. For many days her entire waking life was preoccupied with thoughts and ideas of the preceding hypnotic session. As already mentioned, despite

the fact that she was an excellent somnambule, she did not have amnesia for hypnotic interviews.

As is to be expected in a case of anxiety hysteria, the patient's defense was basically strong repression. Presumably the conversion symptoms represented the points where repression alone was not an adequate defense. There seems to be no doubt that the hypnotic state is somehow able to break through the defense of repression probably in a way related to the phenomenon of hypnotic hyperamnesia. The patient would frequently say, as though groping for a memory or an idea, "I know it's in my mind, but I just can't get it." She recalled one day, for example, that when she was five years old she had fallen off a porch and had broken her arm—a memory that she had long forgotten. She felt that there was something more but couldn't bring it into consciousness. In the next hour she continued to search for the elusive recollection that was just beyond her reach until she was almost in a frenzy of impatience. Suddenly, with climactic force, it burst on her that she had thrown herself from the porch in order to gain her father's attention and had accidentally broken her arm. She felt there was still more and was not satisfied until she had recaptured the final link in the memory which was that she was trying to call her father away from her mother who was in childbirth in the house.

Sometimes she was very reluctant to attempt to pursue a certain topic and then we would insist that she would be able to recall it, employing the various devices of counting, searching for words, etc. We broke through the resistance then, not by interpretations of the reasons for her loss of memory, nor by attempts to reach the memory through uncovering successive layers of superimposed screen memories, but rather by asking for direct recall. That the recall was forthcoming we feel must be a function of the hypnotic state.

We think that in the hypnotic state the ego and the resistances can be temporarily suspended to gain repressed material and that then, within the hypnotic state, this material can be reintegrated into the ego. The patient's spontaneous shifting from past to present tense, which we previously described, can

be viewed as a switching, first out of and then back into the current ego orientation. How it is that hypnosis enables more direct access to the unconscious and the repressed, we do not know. It may be that it is by way of the revival of past ego orientations in which the repression had not yet taken place. We speculate that an important factor permitting this revival of past ego orientations lies in the patient's partial assigning of responsibility for her feelings and verbalizations to the hypnotist. But this abdication of responsibility is a two-edged sword. It enables the repressed material to appear, but for purposes of therapy it must not be permitted to divorce the ego from the proceedings or else the assimilation into the personality of the newly gained insight will not take place. We believe that the analysis of the transference in hypnotic psychotherapy will nevertheless be effective. Unfortunately these short forms must usually deal only with superficial matters and overlook many of the transference manifestations. We believe that in some types of cases this hypodynamic technique will enable more rapid uncovering and resolution of the deeper problems and more adequate dealing with the transference than short psychotherapy permits, but without sacrificing the advantage of brevity.

We make no sweeping claims for this method of treatment and indeed would like to stress our recognition that it can be properly evaluated only by constantly bearing in mind that we dealt with a case of hysteria. The chief mechanism in hysteria is repression and the hypnotic state seems to be peculiarly effective in counteracting that form of defense. Nevertheless, we believe that there is sufficient warrant to investigate this method in other neuroses and we believe that as a technique in the rational therapy of hysteria, it offers great promise.

In summary, modern psychotherapeutic and psychoanalytic techniques can be combined with hypnosis in a treatment method which may prove to be, for some types of cases, a speedy yet effective therapy.

CONCERNING THE NATURE AND CHARACTER OF POST-HYPNOTIC BEHAVIOR

MILTON H. ERICKSON AND ELIZABETH MOORE ERICKSON

Eloise Hospital

Despite the general familiarity of post-hypnotic behavior and its extensive rôle in both experimental and therapeutic work, little recognition has been given to it as a problem complete in itself. Instead, attention has been focused almost exclusively upon the various activities suggested to the subjects as post-hypnotic tasks, with little heed given to the nature of the behavior characterizing, if not constituting, the post-hypnotic state, and which influences and perhaps determines the nature and extent of the suggested post-hypnotic performance. Emphasis has been placed primarily upon the results obtained from post-hypnotic suggestions and not upon the character or nature of the psychological setting in which they were secured. The study of the mental processes and the patterns of behavior upon which those results are based and which must necessarily be in effect in some manner previous to, if not also during, the post-hypnotic performance, has been neglected. Yet, despite a lack of adequate experimental provision, there has been a general recognition of certain significant facts regarding the post-hypnotic performance, which imply directly the existence of a special mental state or condition constituting the background out of which the post-hypnotic act derives.

Foremost among these facts is the occurrence of the post-hypnotic act in response to a suggestion which is remote from the situation in which it has its effect. Next, the immediate stimu-

lus, post-hypnotic signal or cue eliciting the post-hypnotic act serves only to establish the time for the activity and not the kind of behavior, since this is determined by other factors. Also, the post-hypnotic act is not consciously motivated but derives out of a remote situation of which the subject is not consciously aware. Finally, it is not an integrated part of the behavior of the total situation in which it occurs, but is actually disruptive of the conscious stream of activity, with which it may be entirely at variance.

In a search of the literature published during the past 20 years, covering approximately 450 titles, no references were found which were suggestive of a direct study of post-hypnotic behavior itself, although many of the titles indicated that post-hypnotic suggestion had been used to study other patterns of behavior. Similarly, a review of approximately 150 selected articles and books, some of which were published as early as 1888, yielded only a little information definitive of post-hypnotic behavior as a specific phenomenon.

The more instructive references were found chiefly in the general textbooks on hypnotism rather than in experimental studies involving the use of post-hypnotic behavior. However, even these were general assertions or brief, vague, and sometimes self-contradictory statements, based either upon the author's own experience and that of others, or upon experimental material of an inadequate and often irrelevant character in which there was a marked confusion of the results of suggested post-hypnotic activities with the mental processes and patterns of post-hypnotic behavior by which those results were obtained.

Nevertheless, despite their inadequacies, the references found did indicate that there had been frequent recognition of post-hypnotic behavior as constituting a phenomenon in itself, and a number of these will be cited and discussed briefly, with emphasis placed primarily upon those points we propose to develop in direct relation to experimental data in the body of this paper.

Thus, Bernheim (1, p. 157), in discussing post-hypnotic activities, states, "I have said that somnambulists who are susceptible to suggestions *à longue échéance* are all eminently suggestible, even in the waking condition; they pass from one state of con-

sciousness into the other very easily; I repeat the fact that they are somnambulists spontaneously, without any sort of preparation," but he offers no elaboration of this statement.

Likewise, Sidis (13, p. 174) gives recognition to the fact that post-hypnotic behavior is a thing apart from ordinary conscious behavior and is marked by special characteristics. He declares, "The post-hypnotic suggestion rises up from the depths of the secondary self as a fixed, insistent idea. . . . In hypnosis the suggestion is taken up by the secondary, subwaking, suggestible self, and then afterward this suggestion breaks through the stream of waking consciousness. . . ." Without attempting to develop these points, he proceeds with a discussion of certain experimental results, actually irrelevant to these observations.

Similarly, Bramwell (3, p. 95) states:

Under ordinary circumstances, the instant hypnosis is terminated all the phenomena which have characterized it immediately disappear. In response to suggestion, however, one or more of these phenomena may manifest themselves in the subject's waking life. This is brought about in two ways. (1) Where the operator suggests that one or more of the phenomena shall persist after waking. . . . (2) The most interesting class of post-hypnotic suggestions, however, are those in which the appearance of the phenomena has been delayed until some more or less remote time after the termination of hypnosis.

Later in the same chapter, Bramwell (3, pp. 111-112) states, "According to most authorities, post-hypnotic suggestions, even when executed some time after awakening, are not carried out in the normal condition; there is, in effect, a new hypnosis or a state closely resembling it." He proceeds:

According to Moll, the conditions under which post-hypnotic acts are carried out vary widely. He summarizes them as follows: (1) A state in which a new hypnosis, characterized by suggestibility, appears during the execution of the act, with loss of memory afterwards and no spontaneous awakening. (2) A state in which no symptoms of a fresh hypnosis are discoverable although the act is carried out. (3) A state with or without fresh susceptibility to suggestion, with complete forgetfulness of the act and spontaneous awaking. (4) A state of susceptibility to suggestion with subsequent loss of memory.

Apparently, Bramwell approves of this fairly adequate, though confusingly worded, recognition by Moll of the existence

of a post-hypnotic state. Nevertheless, he continues his discussion with an irrelevant exposition of the immediate results obtained through post-hypnotic suggestion in the treatment of physiological disturbances. Except for other similar unsatisfactory and scattered references, he makes no further effort to elaborate his points or those he emphasizes from Moll.

Schilder and Kauders (12, p. 64) offer the following statements which, by their somewhat contradictory nature, serve to emphasize that the post-hypnotic state is of a special character, but that it is hard to recognize:

Certain authors actually assume that the hypnosis again comes to life during the execution of the post-hypnotic command, an assumption which is justifiable to the extent that in a number of such cases the persons experimented on actually do enter into a dream-like state while executing the post-hypnotic order. In other cases, the person complying with the post-hypnotic order can hardly be distinguished from any other person carrying out an order, so that it would be far-fetched to speak of a renewal of the hypnotic state.

No further effort is made to develop these points, except by a general discussion of some results obtained through post-hypnotic suggestion.

Binet and Féré (2, p. 177) recognize that subjects show a peculiar sensitivity to suggestion after awakening from a trance, and they direct attention to post-hypnotic behavior as a specific phenomenon, placing emphasis upon this highly significant observation, ". . . when a subject remains under the influence of a suggestion after awaking he has not, whatever be the appearance to the contrary, returned to his normal state."

Hull (6, p. 300), in direct relation to this passage, takes exception to their declaration, commenting, "This statement is similarly ambiguous from our present point of view because acts performed by post-hypnotic suggestion constitute a special case, as is shown by the fact that they are usually followed by waking amnesia of the acts in question." Just how this comment applies to that observation by Binet and Féré is uncertain. Although Hull, in his textbook published in 1933 (6), does recognize that post-hypnotic behavior is a "special case," he disregards his own statement, as well as his awareness of the observation by Binet and

Féré. Neither he nor his associates make any attempt in their extensive experimental work to provide for the possible existence of any special post-hypnotic state which might have a significant bearing upon post-hypnotic activities. Rather, they persistently limit their investigations of post-hypnotic phenomena to studies of results obtained from suggested activities, without regard for the possible influence upon the assigned task of the mental processes and patterns of behavior peculiar to the post-hypnotic state, and which might significantly, although perhaps indirectly, control the entire character of the post-hypnotic performance. For example, quoting the work of various experimenters, Hull devotes an entire chapter of his textbook to post-hypnotic phenomena, but limits the chapter to studies of amnesia for directly suggested activities and of the durability of post-hypnotic commands, with no reference to that mental state or condition of which the retention and execution of suggestions constitute only a partial reflection.

Nor are Hull and his associates alone in this regard, since it is a general tendency to study post-hypnotic behavior only in terms of how well some suggested task is done, without regard for the mental state or psychic condition constituting the setting for that task. There seems to be no general recognition of the fact that the task performance is only a partial manifestation of the general mental state and not until adequate provision is made for the needs of the situation in which the task is to be done can it be considered a measure of the capacity for performance.

In our judgment, it is this oversight of the special character of the post-hypnotic state that accounts in large part for the confused, unreliable, and contradictory nature of the results obtained in experimental studies of post-hypnotic phenomena.

Thus, in his study of functional anesthetics, Lundholm (9, p. 338) states, "The experiments were carried out with the subject in a post-hypnotic, fully waking condition, but in which he was deaf for the sound-click, the deafness being due to preceding suggestion during hypnotic sleep." An assumption is thereby made that the subject was fully awake and not in either a partially waking or a somnambulistic state, and there is no recog-

dition of the fact that the suggestions given served to effect an actual continuance of the significant part of the trance state, since the post-hypnotic suggestion compelled an uninterrupted persistence of certain phenomena of the trance state, not possible in a fully waking condition.

Another instance in which there is a complete disregard for the post-hypnotic state and a confusion of somnambulistic states with the waking condition may be found in Platanow's (11) experiment on age regression. In describing his experiments he states:

After the subject had reached a suitable state of hypnosis, we generally addressed him as follows:

"At present you are six years old." (This suggestion was repeated three times.) *"After you wake up you will be a child of six. Wake up!"*

After the subject was awake, a short conversation was held with him, for orientation purposes, and this was followed by tests according to the Binet-Simon method. By means of suggestion the subjects were transferred to the ages of four, six and ten. When transferred from one age to another they were hypnotized, given the corresponding suggestion, and awakened again. The experiments were generally ended by the suggestion of the real age, and were followed by amnesia.

From this description, one is led to believe that the subjects were awake in the ordinary sense of the word during the administration of the psychometric tests, despite the experimenter's recognition of the fact that normal waking memories did not obtain and the fact that his experimental findings proved amply that a mental state other than the normal waking one was elicited by the post-hypnotic suggestions.

Fortunately, in both of the above experiments, this confused and contradictory use of terms did not affect the validity of the findings or the conclusions.

A search of Hull's articles, as well as those of his associates, shows many references to the problems involved in studying the outcome of the post-hypnotic state, but there is no apparent realization that the subject, as a consequence of receiving post-hypnotic suggestions or in executing post-hypnotic acts, might manifest behavior apart from the assigned task, which could alter the task performances significantly. Thus, he proposes studies

of learning behavior in response to post-hypnotic suggestion or of amnesias as post-hypnotic phenomena, without any provision for the possible effect, direct or indirect, which the post-hypnotic state might have upon the behavior elicited (7). He is interested, apparently, only in the results secured, and he does not seem to realize that any interpretation of those results must be made in specific terms of the psychological setting in which they were obtained rather than in a categorization as broad and ill-defined as is the term "post-hypnotic." The tendency to regard the results as representing a post-hypnotic performance is of value to that extent only, but does not give any understanding of what the post-hypnotic state itself is, and it accounts largely for the variability of the findings of post-hypnotic investigations. In brief, Hull, as well as others associated with him, emphasizes only post-hypnotic suggestions and their ultimate results and not the post-hypnotic state that must be in existence prior to, if not actually during, the post-hypnotic activity. They disregard entirely the fact that there must necessarily be some state of mind which permits a coming forth into consciousness, or partial consciousness, of the post-hypnotic suggestion, of which, quite frequently, no awareness can be detected in the subject until after the proper cue is given. Even then, that awareness is of a peculiar, limited and restricted character, not comparable to ordinary conscious awareness. Yet, Hull and his associates have directed their attention exclusively to the beginning and the end of a long, complicated process and have disregarded the intermediary steps.

To illustrate the confusion which exists in the use of post-hypnotic suggestion, the experiment by Williams (14, p. 324), among others, may be cited. In his report, Williams states:

In the case of the combined trance-normal work-periods, the subject was awakened when he had reached exhaustion in the trance by repeating rapidly, "*One, two, three—wide awake.*" The instruction to "keep on pulling" was also added in this case so that the subject would continue his work, if possible, in the waking state.

In this combination of instruction in the trance state to awaken, with the command to keep on pulling after awakening, Williams actually gave his subjects a post-hypnotic command. Hence, the

"waking performance" was in response to an unintentional and unrecognized post-hypnotic suggestion. Furthermore, Williams apparently assumed that an awakening from a hypnotic trance could be accomplished instantly, despite a continuance of trance activity, and similarly, in the same experiment, he assumed that a trance induction could also occur instantly without any interruption of waking activities. Hence, the validity of his findings as representing performances in waking and in trance states is to be questioned.

This same confusion of ideas with regard to post-hypnotic suggestion and the results to be expected from it is also shown by Messerschmidt (10) in her experiment on dissociation. Post-hypnotic commands were given in direct and indirect relation to separate tasks, one of which was, presumably, to be done at a conscious level of awareness and the other as a post-hypnotic or "subconscious" performance. As a consequence, both the post-hypnotic behavior and the supposedly waking behavior became integral parts of a single performance, one part of which was provided for by direct post-hypnotic suggestions. The other part was in response to indirect and unintentional post-hypnotic suggestions, specifically, the instruction that the post-hypnotic activity was to be carried on regardless of the assignment in the waking state of a new and different task. Thus, the post-hypnotic suggestions served to instruct the subjects to prepare themselves for certain definite tasks as well as for other tasks not yet specified, although, as the experimental procedures were repeated on them, the subjects necessarily became aware in the trance state that the desired performances were to be dual in character. To instruct a subject in the trance state to execute a given task after awakening, when the subject has full knowledge also of the fact that a second task, contingent upon the first, will be imposed upon him in the waking state, is actually a method of giving two types of post-hypnotic suggestion. Also, to instruct a subject in the trance state that, upon awakening, he is to do serial addition by automatic writing without regard for any other task which may be given him, or to do serial addition "subconsciously" while reading aloud "consciously," constitutes the giving of post-hypnotic

suggestions covering both activities, and, hence the "conscious" task actually becomes a post-hypnotic performance concomitant with the other post-hypnotic activities. Likewise, to suggest to the hypnotized subject that he will do one task "subconsciously" and another task "consciously" will serve only to elicit post-hypnotic performances of both tasks and not a waking performance of one, despite the greater degree of conscious awareness of it, which itself constitutes an additional post-hypnotic response.

Also, in addition to the oversights already mentioned, Messerschmidt's experiment, like Williams', makes no provision for the possible existence of a post-hypnotic state, a somnambulistic state, or any special mental state that might interfere in some way, or exercise a significant influence upon the performance of the suggested tasks.

Quite different from the usual experimental study of post-hypnotic behavior is the report by Brickner and Kubie (4), who emphasize throughout their investigation the significant effect which the mental state that develops directly from post-hypnotic suggestions has upon the total pattern of behavior. They also note the disappearance of those changes in the general behavior upon the completion of the post-hypnotic task.

Similarly, although their studies were directed primarily to other purposes, Erickson (5), and Huston and his co-workers (8) demonstrate clearly the development, in direct consequence of post-hypnotic suggestion, of a special mental state or condition which influences, alters, and even negates the subject's ordinary waking behavior in routine situations until the post-hypnotic suggestion has been either removed or acted upon completely.

While this review of the literature is necessarily incomplete, it does disclose that there has been frequent, if inadequate, recognition as well as complete disregard of the special mental state that develops in direct relation to post-hypnotic suggestions, and which is not necessarily limited to the task suggested as the post-hypnotic activity. Also, it shows that much experimental work has been done on post-hypnotic behavior with no attempt made either to define the post-hypnotic state or to make provision for any significant bearing it might have upon experimental pro-

cedures. Neither has there been any attempt to give an adequate definition of the post-hypnotic act specifically, except in terms of the results secured from it. The mental processes and the patterns of response by which those results were achieved have been ignored. Instead, there has been the general assumption that the post-hypnotic act is simply a performance elicited in response to a command given during the trance state and characterized variously and uncertainly by degrees of amnesia, automaticity, and compulsiveness. As a consequence of the inadequate determination of the exact nature and character of a post-hypnotic act, much experimental work has led to unsatisfactory and conflicting results, and hence, there is a need for more definitive studies of post-hypnotic behavior as a specific phenomenon rather than as a means by which to study other mental processes.

We propose, therefore, to report in this paper various significant observations, both general and specific, upon the nature and character of post-hypnotic behavior. These observations have been made by us repeatedly and consistently during the course of experimental and therapeutic work extending over a period of years, and we have also verified our findings by inquiry into the experience of others and by direct observation of the post-hypnotic behavior of subjects employed by other hypnotists.

A DEFINITION OF THE POST-HYPNOTIC ACT

We have found the following definition of the post-hypnotic act to be consistently applicable and useful, since it serves to describe adequately a form of behavior we have elicited innumerable times in a great variety of situations and from a large number of subjects, ranging in type from the feeble-minded to the highly intelligent, from the normal to the psychotic, and in age from children to middle-aged adults. For the moment, we shall limit this definition strictly to the act itself, without regard for partial performances resulting from light trances or for certain other important considerations which will be discussed later. *A post-hypnotic act has been found to be one performed by the hypnotic subject after awakening from a trance, in response to*

suggestions given during the trance state, with the execution of the act marked by an absence of any demonstrable conscious awareness in the subject of the underlying cause and motive for his act. We have come to regard as valid this form of the post-hypnotic act since its performance is invariably characterized by definitive and highly significant attributive behavior.

THE BEHAVIOR CHARACTERIZING THE POST-HYPNOTIC PERFORMANCE

This important attributive behavior belonging to the post-hypnotic response consists of *the spontaneous and invariable development, as an integral part of the performance of the suggested post-hypnotic act, of a self-limited brief, hypnotic trance.* In other words, we have observed repeatedly, under varying circumstances and in a great variety of situations, that the hypnotized subject, instructed to execute some act post-hypnotically, invariably develops spontaneously a hypnotic trance. This trance is usually of brief duration, occurs in direct relation to the performance of the post-hypnotic act, and apparently constitutes an essential part of the process of response to and execution of the post-hypnotic command. Its development has been found to be an invariable occurrence despite certain apparent exceptions, which will be discussed later, and regardless of the demands of the post-hypnotic suggestion, which may entail a long, complicated form of behavior, the introduction of a single word into a casual conversation, the development of an emotional response or attitude at a given stimulus, an avoidance reaction or even a slight modification of general behavior. Furthermore, the development of a trance state as a part of the post-hypnotic performance requires for its appearance neither suggestion nor instruction. This special trance state occurs as readily in the naive as in the highly trained subject; its manifestations, as we shall show, differ essentially in no way from those of an ordinary induced trance; and it seems to be a function of the process of initiating in the immediate situation a response to the post-hypnotic suggestion given in a previous trance.

THE GENERAL CHARACTER OF THE SPONTANEOUS POST-HYPNOTIC TRANCE

The spontaneous post-hypnotic trance is usually single in appearance, develops at the moment of initiation of the post-hypnotic act, and persists usually for only a moment or two; hence, it is easily overlooked, despite certain residual effects it has upon the general behavior. Under various circumstances, and with different subjects, however, the trance may be multiple in appearance, constituting actually a succession of brief spontaneous trances related to aspects or phases of the post-hypnotic act. It may appear in a prolonged form and persist throughout the greater part or even the entire duration of the post-hypnotic performance; or there may be an irregular succession of relatively short and long spontaneous trances, apparently in relation to the difficulties, mental and physical, encountered in the course of the execution of the post-hypnotic act. In general, any variation in the form or the time of its appearance or reappearances seems to be a function of individual differences in the subjects and of the difficulties occasioned by the general situation or by the post-hypnotic act itself.

SPECIFIC MANIFESTATIONS OF THE SPONTANEOUS POST-HYPNOTIC TRANCE

The specific hypnotic manifestations which develop in relation to the performance of the post-hypnotic act form an essentially constant pattern, although the duration of the separate items of behavior varies greatly both in accord with the purpose served and with the individual subject. They occur rapidly in direct relation to the giving of the specified cue for the post-hypnotic act, with a tendency toward the following sequence: A slight pause in the subject's immediate activity, a facial expression of distraction and detachment, a peculiar glassiness of the eyes with a dilatation of the pupils and a failure to focus, a condition of catalepsy, a fixity and narrowing of attention, an intentness of purpose, a marked loss of contact with the general environment, and an unresponsiveness to any external stimulus until

the post-hypnotic act is either in progress or has been completed, depending upon the actual duration of the trance state itself and the demands of the post-hypnotic task. Even after the trance state has ceased, these manifestations, somewhat modified, continue as residual effects upon the subject, and result in the intent, rigid, and almost compulsive nature of his behavior, and his state of absorption and general unresponsiveness until he has reoriented himself to the immediate situation.

Similarly, to a slight degree, the disappearance of the trance state, or, to a much greater degree, the completion of the post-hypnotic performance, is marked by a brief interval of confusion and disorientation from which the subject quickly recovers by renewed and close attention to the immediate situation. Especially does this confusion and disorientation become marked if, during the state of absorption in the post-hypnotic performance, there occurred any significant change or alteration in the general situation. In addition, there is usually evidence of an amnesia, either partial or complete, for both the post-hypnotic act and the concurrent events arising out of the immediate situation. In those instances in which the subject does have a recollection of the course of events, investigation will disclose his memories to be hazy, faulty, and frequently more deductions than memories, based upon his interpretations and rationalizations of the situation to which he has reoriented himself. Occasionally, however, despite a poor recollection of or a complete amnesia for the attendant circumstances, a subject may recall clearly the entire post-hypnotic performance, but will regard it merely as an isolated, unaccountable, circumscribed impulsion, or, more often, a compulsion having no connection with the immediate or general situation.

An example illustrative of many of these points is the following account given in a hesitating, uncertain fashion by a subject upon the completion of a post-hypnotic act:

We were talking about something, just what I've forgotten now, when I suddenly saw that book and I simply had to go over and pick it up and look at it—I don't know why—I just felt I had to—a sudden impulse, I suppose. Then I came back to my chair. It just happened that

way. But you must have seen me because I must have had to walk around you to get it—I don't see any other way I could have reached it. Then when I laid it down again, I must have put those other books on top of it. At least, I don't think anybody else did, since I don't remember anybody else being on that side of the room—but I wasn't paying much attention to anything, I guess, because, although I know I looked carefully at that book and opened it, I don't even know the author or the title—probably fiction from the looks of it. Anyway, it was a funny thing to do—probably an impulse of the moment and doesn't mean a thing. What was it we were discussing?

THE DEMONSTRATION AND TESTING OF THE SPONTANEOUS POST-HYPNOTIC TRANCE

Although the various forms of hypnotic behavior spontaneously manifested by the subject in relation to post-hypnotic acts constitute actually a demonstration of a trance state, their brevity and self-limited character necessitate special measures for a satisfactory examination of them and for a testing of their significance.

This may be done readily without distorting or altering significantly the actual hypnotic situation, since the giving of the post-hypnotic cue or signal serves to re-establish that state of rapport existing at the time the post-hypnotic suggestion was given. The task of such a demonstration, however, as experience will show, requires a considerable degree of skill. Usually it is most easily and effectively done by some form of interference, either with the post-hypnotic act itself or with the subject after the post-hypnotic response has been initiated but not yet completed. The demonstration of the trance state may follow one of two courses, depending upon the presence or the absence of hypnotic rapport between the demonstrator and the subject. If there be a state of rapport, the interference may be directed either to the subject or to his performance, and the trance manifestations are of the positive responsive type, characteristic of the relationship between hypnotist and subject. In the absence of rapport, effective interference must be directed primarily to the act itself and the trance manifestations are of the negative, unresponsive type, characteristic of the hypnotized subject's unresponsiveness to

and detachment from that which is not included in the hypnotic situation. In both instances, however, the general and specific behavior obtained is wholly in keeping with that which would be obtained under similar circumstances from the same subject in an ordinary induced hypnotic trance.

The interference most effective in demonstrating the trance is that offered by the hypnotist or by some person actually in rapport with the subject when the post-hypnotic suggestion was given in the original trance. It is best accomplished at the exact moment of initiation of the post-hypnotic response by some measure serving to counteract or to alter the original post-hypnotic suggestion, or to compel the subject to give special attention to the hypnotist, as, for example, the deliberate removal of the object which the subject was instructed to examine; the manipulation of the subject in such a fashion as to effect the development of catalepsy in one or both arms, thus rendering the examination difficult or impossible, or the use, even with naive subjects who have had no previous training, of such vague verbal suggestions as, *"Wait a moment, just a moment," "Don't let anything change now," "Stay as you are right now, never mind that," "I'd rather talk to you now,"* or, *"I will be waiting as soon as you have done it,"* and similar remarks implying that an additional assignment may be made.

The effect of such interference is usually a complete arrest of the subject's responses followed by an apparent waiting for further instructions, while his appearance and manner suggest a state identical with that of the deep trance as ordinarily induced, and all the customary phenomena of the deep hypnotic trance can be elicited from him. Then, if he is allowed to return to the performance of the post-hypnotic task, a spontaneous awakening will ensue in due course, permitting an immediate and direct contrast of waking and hypnotic behavior as well as a demonstration of an amnesia for the post-hypnotic act, the interference, and the events of the trance state. If, however, no use is made of the peculiar state of responsiveness established by the interference with him, the subject tends to return to the problem of the post-hypnotic task. The sequence of his behavior thereafter is essen-

tially as if there had been no interference, but there is then a marked tendency for the spontaneous trance state to persist until the post-hypnotic task has been completed. Especially is this true if the interference has rendered the task more difficult. Occasionally, however, instead of being arrested in his behavior, the subject may proceed uninterruptedly with his post-hypnotic task, and, upon its completion appear to be awaiting further instruction. The phenomena of the deep trance state can then be elicited, but if this is done, it becomes necessary to awaken the subject at the finish.

To illustrate briefly, since other examples will be given later, a subject was told that, shortly after his awakening, a certain topic of conversation would be introduced, whereupon he was to leave his chair immediately, cross the room and, with his left hand, pick up a small statuette and place it on top of a certain bookcase. At the proper time, as the subject stepped in front of the hypnotist to cross the room, his left arm was gently raised above his head, where it remained in a cataleptic state. The subject continued on his way without hesitation but, upon approaching the statuette, he apparently found himself unable to lower his left arm and turned to the hypnotist as if awaiting further instruction. Thereupon, he was used to demonstrate a variety of the usual phenomena of the ordinary induced trance. Upon the completion of this demonstration, he was instructed simply, "*All right, you may go ahead now.*" In response to this vague suggestion, the subject returned to the interrupted post-hypnotic performance, completed it, and resumed his original seat, awakening spontaneously with a complete amnesia for all of the events intervening between the giving of the cue and his awakening and without even an awareness that he had altered his position in the chair.

This same procedure of interference was repeated upon another subject with essentially the same results. When, however, the hypnotist made no response to the subject's expectant attitude, there occurred a fairly rapid disappearance of the catalepsy, a performance of the task, and a return to his seat, followed by a spontaneous waking with a complete amnesia for the entire experience.

SPECIAL TYPES OF SPONTANEOUS POST-HYPNOTIC TRANCE
BEHAVIOR

In those instances in which the interference is not given at the proper moment, while it usually has the effect of intensifying and prolonging greatly the duration of the spontaneous trance, the subject may respond to it by bewilderment and confusion succeeded by a laborious compulsive performance of the post-hypnotic act and an overcoming of the interference. Again, he may misinterpret the interruption of his task as a coincidental and meaningless, though obstructive, occurrence which is to be disregarded; or he may behave as if there really had been none.

This last type of behavior is of a remarkable character. It appears in other connections than the situation of mistimed interference, and may serve widely different purposes for the same or different subjects. Thus, it may occur when the interference is limited to the purpose of demonstrating the trance state without affecting the actual performance of the post-hypnotic act. In this case, the subject merely ignores the most persistent efforts on the part of the hypnotist, completes his post-hypnotic task, and awakens spontaneously with a total amnesia for the entire occurrence. Frequently, it develops when the possibility of the post-hypnotic act has been nullified; and it often appears when the post-hypnotic suggestion is rendered objectionable in character to the subject or too difficult as a result of the interference. But of most interest is its tendency to occur almost invariably when, upon the initiation of the post-hypnotic behavior, some person, not in rapport with the subject, intrudes into the situation by means of an interference directed primarily to the post-hypnotic act.

Although these situations differ greatly, the pattern of the subject's behavior is essentially the same for all of them, and the general course of the subject's responses in each type of situation is adequately exemplified in the following accounts: At the previously established post-hypnotic cue, the subject glanced across the room at an easily visible book lying on a table and proceeded to rise from his chair for the purpose of securing the book and placing it in the bookcase in accord with the previously given post-

hypnotic instructions. As he shifted his position in his chair, preparatory to rising, an assistant, not in rapport with the subject, quickly removed and concealed the book, this being done at a moment when the subject's gaze was directed elsewhere. Despite this absolute interference with the post-hypnotic act, the subject unhesitatingly performed the task by apparently hallucinating the book, and gave no evidence of any realization that something unusual had occurred. This same procedure, repeated with other subjects, has led in more than one instance to an even more hallucinatory and delusional response, namely, upon actually noting that the book had vanished, glancing at the bookcase in a bewildered fashion, and then apparently hallucinating the book in the place suggested for it and assuming that they just completed the task. As one subject spontaneously explained:

It's funny how absent-minded you can get. For a minute there I intended to put that book in the bookcase, when actually I had just finished doing so. I suppose that's because it annoyed me so much just lying there that the thing before my mind was the doing of it, and that I hadn't got around yet to knowing that I had already done it.

Yet, upon resuming her seat, she spontaneously awakened and demonstrated a total amnesia inclusive even of her explanatory remarks.

Repetition of the procedure with these and with other subjects, but with the removal of the book effected while the subject's gaze was directed at it, sometimes led to similar results in that the removal of the book was not detected, thereby indirectly disclosing the defectiveness of the hypnotic subject's contact with the external environment and the tendency to substitute memory images for reality objects, behavior highly characteristic of the hypnotic state. In other instances, the new position of the book was detected and the original position regarded as an illusion. Also, in some instances, plausible misconstructions were placed upon the new position or the detected movement, as, for example: *"Why, who left this book lying in this chair? I remember distinctly seeing it on the table,"* or, *"I've been expecting that book to slip off the pile on the table all evening and at last it has. Do you mind if I put it in the bookcase?"* And, depending upon the

actual experimental situation, the real or an hallucinatory book would be recovered from the chair or the floor, and the post-hypnotic act would be performed, with the customary sequence of events.

Following this general type of post-hypnotic behavior, there develops either an amnesia complete in character and inclusive of both the post-hypnotic act and the attendant circumstances, as well as of the subject's interpolated behavior, or, less frequently, a peculiar admixture of amnesia and fragmentary memories. These partial memories often tend to be remarkably clear, vivid, and distracting in character, and they may relate to the absolute facts or even to the hallucinatory and delusional items of the post-hypnotic trance period. For example, the last subject quoted above, when questioned for her recollections, recalled only that the hypnotist had a habit of piling books, papers, folders, and journals in untidy heaps, but she was unable to give a specific example of this practice. Another subject, in a similar experimental situation, remembered most vividly minute and utterly irrelevant details about the goldfish in the fish globe used only as a part of the environmental setting for the post-hypnotic act, and he was most insistent that these memories constituted a complete account of the entire occurrence. Nevertheless, some weeks later the subject disclaimed any memory of having made such statements.

THE EFFECT OF TIME UPON THE DEVELOPMENT OF THE SPONTANEOUS POST-HYPNOTIC TRANCE

One other general consideration in relation to the development of a spontaneous trance upon the initiation of post-hypnotic behavior concerns the possible effect of the lapse of time. In this regard, on a considerable number of occasions, subjects have been given specific instructions in the form of a post-hypnotic suggestion to perform some simple act, the nature of which varied from subject to subject. This act was to be "done without fail on the occasion of our next meeting." Among these subjects were some who were not seen after the giving of such post-hypnotic suggestions for varying periods of months. Of this group, all carried

out the post-hypnotic act, developing as they did so a spontaneous trance. Two other subjects were actually not seen until three years later and another two were not seen for four and five years respectively, during which periods of time there was no form of contact between the hypnotist and the subjects. Nevertheless, at chance meetings with them, the performance of the post-hypnotic act and the development of a concomitant spontaneous trance state occurred.

APPARENT EXCEPTIONS TO THE RULE OF SPONTANEOUS POST-HYPNOTIC TRANCES

However, before continuing with a discussion of various significances of the spontaneous post-hypnotic trance, it may be well to offer an explanation of the apparently absolute exceptions, mentioned previously, to the development of a spontaneous trance in relation to the execution of post-hypnotic suggestions.

These exceptions, in which there is a performance post-hypnotically of the trance-suggested act without the apparent development of a spontaneous trance, arise usually from certain conditions which will be listed generally and illustrated as follows:

1. Failure of the development of an amnesia for the post-hypnotic suggestions: In this situation there may be actually no post-hypnotic performance as such, since the subject understands from the beginning the underlying motivations and cause of his behavior, and hence, acts at a level of conscious awareness. Consequently, the performance becomes similar in character to one suggested to a person in the ordinary waking state and it is post-hypnotic only in its time relationships.

In such instances, the act is essentially voluntary in character, although frequently another element may enter into the situation, namely, a sense of being compelled to perform the specified task, despite the subject's apparently complete understanding of the situation. Thus, the subject may remember his instructions and be fully aware of what he is to do and why he is to do it, and yet experience an overwhelming compulsion that causes him to perform the act with literally no choice on his part. Occasionally,

however, the subject, in responding to this compulsion and executing the post-hypnotic instructions, develops, as he performs the task, a spontaneous trance. This trance often serves to establish for the subject a more or less complete amnesia for the instructions, for the period of waiting with its usually unpleasant compulsive feelings, and for the act itself. The trance is similar in character to that which develops in the ordinary post-hypnotic situation, with the exception that the amnesia it may cause tends to be more limited. Thus, the subject may remember the post-hypnotic suggestions, the period of waiting and the feeling of compulsion, but have a complete amnesia for his actual performance. Or he may develop an amnesia for the post-hypnotic instructions but remember experiencing a compulsion to perform an apparently irrational act. However, in some instances, the spontaneous trance serves as a defense mechanism against the compulsive feelings rather than as an essential or integral part of the atypical post-trance performance. Finally, the development of compulsive feelings constitutes a marked alteration of the essential nature of the entire pattern of behavior.

2. Failure to make clear to the subject that the post-hypnotic instructions given concern the act itself and not the process of making provision for such an act: Thus, the subject, instructed to perform a certain task post-hypnotically, may, after awakening, go through a mental process of realizing, sometimes vaguely, sometimes clearly, that a certain act is to be performed and then simply hold himself in readiness for that act. Hence, upon the performance of the task, no spontaneous trance occurs. However, this does not constitute a negation of the statement that a spontaneous trance always accompanies the post-hypnotic performance, since close observation of the subject in this situation will disclose that a spontaneous trance invariably accompanies this process of making ready for the act, provided that this understanding of his task occurs definitely after the subject has awakened from the trance in which the suggestion was given and not while he is going through a slow process of awakening, in which case the situation would become similar to that of the failure to develop amnesia.

3. Unwillingness on the part of the subject to perform the post-hypnotic act, except as a deliberate act of choice on his part: Thus the subject may, for some reason or whim, object to the purely responsive character of a post-hypnotic performance and react by making his response one of deliberate intention. In this situation, as in the foregoing example, there occurs upon awakening the same process of making ready for the suggested task, and hence, upon the proper signal, the post-hypnotic performance is executed without the development of a spontaneous trance. However, this process of making ready for the act is again accompanied by a spontaneous trance.

4. The failure of the amnesia for the trance experiences: This is the most common and consists essentially in the spontaneous recovery of the memories of the events and experiences of the trance state. For example, the subject instructed to perform a post-hypnotic act at a given time after awakening, may, before the specified time, more or less slowly begin to recall his various trance experiences, among them the post-hypnotic instructions. This process of recollection is not one of preparation for the post-hypnotic performance, but constitutes rather a recovery of memories, motivated usually by a sense of curiosity, and it is free from any purposeful significance in relation to the actual suggested post-hypnotic task. Literally, it is a breaking through of memories because of an inadequacy of amnesic barriers. With the recovery of the memory of the post-hypnotic suggestions a somewhat similar situation obtains as exists when there is a failure of the development of an amnesia for post-hypnotic suggestion, which has been described above. In general, while this type of behavior is the most common, it is exceedingly difficult to understand fully because there is first an amnesia for and then a recollection of post-hypnotic instructions and because the memories, however complete eventually, are recovered in a fragmentary fashion.

Hence, the failure, apparent or absolute, to develop a spontaneous trance upon the initiation of the execution of an act suggested as a post-hypnotic performance does not necessarily constitute a contradiction of our observation. Rather, it implies that there may occur within the subject certain changes in the psycho-

logical situation. These, in turn, may serve to alter or to transform the character of the post-hypnotic act itself and thus to render it one for which the subject has a preliminary awareness as well as an understanding of its underlying nature and cause. Hence, the act becomes transformed into one post-hypnotic in time relationships only.

SIGNIFICANCE OF THE SPONTANEOUS POST-HYPNOTIC TRANCE

The significances of the spontaneous trance state as an integral part of the execution of post-hypnotic suggestions are numerous and bear upon many important hypnotic questions. In particular, they relate to such problems as the establishment of objective criteria for trance states and conditions, the training of subjects to develop more profound trances, and the direct elicitation of various hypnotic phenomena without a preliminary process of suggestion for trance induction. In addition, the post-hypnotic trance bears upon the general problem of dissociation, the various problems of individual hypnotic phenomena, such as rapport, amnesia, selective memories, catalepsy and dissociated states, and the general experimental and therapeutic implications of post-hypnotic phenomena. Discussion of some of these considerations will be given in connection with our investigative work, but the reader will note that the experimental findings serve also to illustrate many points not directly mentioned.

THE SPONTANEOUS POST-HYPNOTIC TRANCE AS A CRITERION OF THE INDUCED HYPNOTIC TRANCE

In relation to the establishment of criteria for trance states, our experience has been that the spontaneous post-hypnotic trance constitutes a reliable indicator of the validity of the original trance, and in this belief we have been confirmed by the experience reported to us by others. Apparently, the post-hypnotic trance is a phenomenon of sequence; it is based upon the original trance and constitutes actually a revivification of the hypnotic elements of that trance. Especially does this inference seem to be warranted since careful observation will often disclose an ab-

solute continuance in the spontaneous post-hypnotic trance of the behavior patterns belonging actually to the original trance state. This may be illustrated by the following experimental findings, made originally by chance and since repeated on other subjects: During a single hypnotic trance, the hypnotist gave a large number of unrelated post-hypnotic suggestions, each of which was to be performed later as a separate task and in response to separate cues. Also, during the course of that trance, the subject's state of rapport with two observers was made to vary from time to time by suggestions independent of the post-hypnotic suggestions. Subsequently, upon the execution of the post-hypnotic suggestions, the spontaneous trance states that developed showed remarkable variations, in that the subject, while always in rapport with the hypnotist, variously manifested rapport with one or the other or both or neither of the two observers. Although this was not understood at the time, subsequent checking of the record disclosed that the state of rapport manifested in each spontaneous post-hypnotic trance state constituted an accurate reflection of the exact state of rapport existing at the time of the giving of the particular post-hypnotic suggestion. Aside from the question of the continuance of patterns of behavior, the bearing of this finding upon the question of rapport is at once apparent.

Since then investigative work has disclosed that proper wording of post-hypnotic suggestions may effect either a continuance or an absence in the spontaneous trance of the general behavior patterns belonging to the trance state in which the post-hypnotic suggestion was given. Thus, the giving of post-hypnotic suggestions so worded as to carry an implication of a change or an alteration of the situation may militate against the evocation of original trance behavior. Yet, the same suggestion so worded as to carry immediate as well as remote implications will usually serve to effect a continuance of the original trance behavior. To illustrate: During experimental work on this problem, it was found that this wording of a post-hypnotic suggestion, "*As I jingle my keys, you will invariably—*," often served to cause a continuance in the spontaneous post-hypnotic trance of the behavior

patterns belonging to the original trance, while, "*Tomorrow, or whenever I jingle my keys, you will invariably—*," would fail in the same subject to elicit the behavior patterns of the original trance, since this wording implied possible changes in the situation. However, extensive work has shown that the behavior of subjects in carrying over the patterns of response belonging to the original trance is highly individualistic. Some almost invariably do so, others seldom or never, some almost wholly, others only in selected relationships, and the outcome of any experimental work is highly unpredictable, depending apparently upon the individuality of the subject as well as his immediate understandings. Hence, extreme care in wording suggestions is highly essential and it should never be assumed that the subject's understanding of instructions is identical with that of the hypnotist. Neither should there be the assumption that an identical wording must necessarily convey an identical meaning to different subjects.

In other words, the "standardized technique," or the giving of identical suggestions to different subjects, described by Hull (6, p. 50), is not, as he appears to believe, a controlled method for eliciting the same degree or type of response, but merely a measure of demonstrating the general limitations of such a technique.

Another type of evidence concerning the validity of the original trance is the failure to develop a spontaneous trance when apparently executing a post-hypnotic suggestion, by subjects who were merely complaisantly coöperative or who were over-eager to believe that they were in a trance, or who, for various reasons, simulated effectively being hypnotized. In direct contrast to these subjects are those relatively rare persons who actually do go into a deep hypnotic trance but who, because of individual peculiarities, seem unable to realize the fact, or are unable to admit it to themselves and hence refuse to believe that they are or ever have been hypnotized. Yet, invariably this latter class of subjects develops a spontaneous trance upon the execution of post-hypnotic suggestions, an occurrence which, in itself, often constitutes an effective measure in correcting their mental attitudes and misunderstandings.

Furthermore, in studies directed to the detection of the simulation of trance behavior, the failure of a trance state to develop upon the execution of post-hypnotic suggestions discloses any simulations. Nor does sophistication and coaching in this regard serve to enable a satisfactory simulation of the spontaneous trance state, since, on many occasions, trained subjects, purposely kept unaware that the performance they were watching was one of deliberate pretense, have declared the apparent performance of a post-hypnotic act to be "*not right*," "*something wrong*," or have stated, "*I don't get the right feeling from the way he did that*," but without being able to define their reasons, since their own post-hypnotic amnesias precluded full conscious understandings.

In brief, on innumerable occasions and under a variety of circumstances, the spontaneous post-hypnotic trance has been found to be characterized by the individual phenomena of the original trance state in which the post-hypnotic suggestion was given, and to be an excellent measure of differentiating between real and simulated trances, especially so when the subject, by being over-coöperative, deceives himself. Likewise, it has been found to be an effective measure in aiding responsive hypnotic subjects who, for personality reasons, cannot accept the fact of their hypnotization. Also, it can be used to demonstrate effectively the individuality and variety of responses that may be elicited under apparently controlled conditions.

THE UTILIZATION OF THE SPONTANEOUS POST-HYPNOTIC TRANCE AS A SPECIAL HYPNOTIC TECHNIQUE

Of particular importance is the utilization of the spontaneous post-hypnotic trance as a special experimental and therapeutic technique. Its usefulness is varied in character and relates to the intimately associated problems of avoiding difficulties deriving from waking behavior, securing new trance states, training subjects to develop more profound trances, and eliciting specific hypnotic phenomena without direct or indirect suggestions made to that end.

The method of utilization is illustrated in the following experimental account: A five-year-old child, who had never wit-

nessed a hypnotic trance, was seen alone by the hypnotist. She was placed in a chair and told repeatedly to "go to sleep," and to "sleep very soundly," while holding her favorite doll. No other suggestion of any sort was given her until after she had apparently slept soundly for some time. Then she was told, as a post-hypnotic suggestion, that some other day the hypnotist would ask her about her doll, whereupon she was to (a) place it in a chair, (b) sit down near it, and (c) wait for it to go to sleep. After several repetitions of these instructions, she was told to awaken and to continue her play. This three-fold form of post-hypnotic suggestion was employed since obedience to it would lead progressively to an essentially static situation for the subject. Particularly did the last item of behavior require an indefinitely prolonged and passive form of response, which would be best achieved by a continuation of the spontaneous post-hypnotic trance.

Several days later she was seen while at play and a casual inquiry was made about her doll. Securing the doll from its cradle, she exhibited it proudly and then explained that the doll was tired and wanted to go to sleep, placing it as she spoke in the proper chair and sitting down quietly beside it to watch. She soon gave the appearance of being in a trance state, although her eyes were still open. When asked what she was doing she replied, "Waiting," and nodded her head agreeably when told insistently, "Stay just like you are and keep on waiting." Systematic investigation, with an avoidance of any measure that might cause a purely responsive manifestation to a specific but unintentional hypnotic suggestion, led to the discovery of a wide variety of the phenomena typical of the ordinary induced trance. A number of these will be cited in detail in the following paragraphs to illustrate both the procedure employed and the results obtained.

Catalepsy

The subject was asked if she would like to see a new toy the hypnotist had for her. Contrary to her ordinary behavior of excited response in such a situation, she simply nodded her head

and waited passively for the hypnotist to secure the new toy (a large doll) from a place of concealment. She smiled happily when it was held up to her view, but made no effort to reach for it. Upon being asked if she would like to hold it, she nodded her head agreeably, but still made no effort to take it. The doll was placed in her lap and the hypnotist then helped her to nestle it in her right arm, but in such fashion that the arm was in a decidedly awkward position. She made no effort to shift the position of her arm, but merely continued to look happily at the doll.

While she was so engaged, the hypnotist remarked that her shoe string was untied and asked if he might tie it for her. Again she nodded her head and the hypnotist lifted her foot slightly by the shoe strings so that the task might be done more easily. When her foot was released, it remained in the position to which it had been elevated.

Following this, she was asked if she would like to put the doll in its cradle. Her only response was an affirmative nod. After a few moments wait, she was asked if she would not like to do so at once. Again she nodded her head, but still continued to wait for specific instructions. Thereupon the hypnotist told her to "*go ahead*," meanwhile picking up a book as if to read. The subject responded by repeated futile attempts to rise from the chair, but the catalepsy present, manifested by the continuance of the awkward position in which she was holding the doll and the elevation of her foot, prevented her from making the shift of position necessary for rising. She was asked why she did not put the doll in the cradle, to which she replied, "*Can't*." When asked if she wanted help, she nodded her head, whereupon the hypnotist leaned forward in such fashion that he pushed her leg down. Taking her by the left hand, he gently pulled her to a standing position with her arm outstretched, in which position it remained upon being released. She immediately walked over to the cradle, but stood there helplessly, apparently unable to move either arm, and it became necessary to tell her to put the doll in the cradle. With this specific instruction, the catalepsy disappeared from her arms and she was able to obey.

Rapport and Hallucinatory Behavior

The subject was then asked to return to her original seat, where she continued to gaze in a passive manner at the first doll in its chair. One of the hypnotist's assistants entered the room, walked over and picked up that doll and removed it to another chair. Despite the fact that the subject had her gaze directed fully at the doll, she made no response to this maneuver, nor did she appear to detect in any way the alteration of the situation. After a few moments, the hypnotist asked her what she was doing. She replied, "*I'm watching my dolly.*" Asked what the doll was doing, she answered simply, "*Sleeping.*" At this point, the assistant called the subject by name and inquired how long the doll had been sleeping, but elicited no response. The question was repeated without results, whereupon the assistant nudged the subject's arm. The subject immediately looked briefly at her arm and scratched it in a casual fashion, but made no other response.

Following this, the assistant secured the two dolls and dropped them into the hypnotist's lap. The subject was then asked if she thought both dolls liked to sleep, thereby causing her to shift her gaze from the empty chair to the hypnotist. She apparently failed to see the dolls in the new position, but when they were picked up and looked at directly by the hypnotist, she immediately became aware of them, glanced hesitatingly at the chair and then at the cradle, and remarked, "*You got them now,*" and seemed to be very much puzzled. Yet, when the assistant quietly took the dolls out of the hypnotist's hands and walked to the other side of the room, the subject apparently continued to see the dolls as if they were still held by the hypnotist. An attempt on the part of the assistant to call the subject's attention to the dolls failed to elicit a response of any sort from the subject.

The subject's mother then entered the room and attempted to attract her attention, but without results. Yet, the subject could walk around, talk to the hypnotist, and see any particular object or person called directly to her attention by the hypnotist, although she was apparently totally unable to respond to anything not belonging strictly to the hypnotic situation.

Amnesia

The others were dismissed from the room, the dolls were restored to the chair and the cradle respectively, and the subject to her seat, whereupon she was told to awaken. Immediately upon manifesting an appearance of being awake, the subject, returning to the initial situation, remarked in her ordinary manner, "*I don't think dolly is going to go to sleep. She's awake.*" She was asked various casual questions about the doll, following which the hypnotist remarked that maybe the doll did not like to go to sleep in a chair. Immediately the subject jumped up and declared her intention of putting the doll in its cradle, but when she attempted to do so she manifested very marked bewilderment at the presence of the new doll in the cradle. There was no recognition of it, no realization that she had ever seen the doll before, and no knowledge that it had been made a gift to her. She showed the typical excited childish desire for the new toy, asking whose it was and if she might have it. The assistant then re-entered the room and picked up the doll whereupon the subject began addressing remarks to the assistant. The assistant, replying to these, walked over to the chair and picked up the first doll. The subject made full and adequate response to this, disclosing complete contact with her surroundings and a complete amnesia for all trance occurrences.

Repetitions of the procedure upon the subject under varying circumstances led to similar findings. Likewise, similar procedures have been employed with other naive and trained subjects of various ages with comparable results.

This general type of technique we have found especially useful both experimentally and therapeutically since it lessens greatly those difficulties encountered in the ordinary process of inducing a trance, which derive from the need to subordinate and eliminate waking patterns of behavior. Once the initial trance has been induced and limited strictly to passive sleeping behavior with only the additional item of an acceptable post-hypnotic suggestion given in such fashion that its execution can fit into the natural course of ordinary waking events, there is then an

opportunity to elicit the post-hypnotic performance with its concomitant spontaneous trance. Proper interference, not necessary in the instance cited above because of the nature of the post-hypnotic performance, can then serve to arrest the subject in that trance state.

However, it must be stated that, to arrest the subject in the spontaneous trance and to have him remain in that state, the entire situation must be conducive to such a purpose, since any unwillingness on the part of the subject will cause him to become unresponsive and to awaken. But under favorable circumstances, the subject submits readily and fully to the new hypnotic situation in a passive responsive fashion. Repeated intensive inquiry of subjects while in such prolonged trance states has disclosed no understanding of how the trance was secured nor any intellectual curiosity about it, and usually little or no spontaneous realization that they are in a trance. Rather, there seems to be only a passive acceptance of their trance state marked by the automatic responsive behavior so characteristic of the ordinary deep induced trance.

By this general measure, new trance states can be secured free from the limitations deriving from various factors such as the subject's mental set, deliberate conscious intentions regarding trance behavior, misconceptions, and the continuance of waking patterns of behavior. Under ordinary circumstances, the hypnotic subject, obeying a post-hypnotic command, is making a response to a suggestion of which he is unaware at a conscious level of understanding, and which belongs to another situation of which he is similarly unaware. In addition, he becomes so absorbed and so automatic in his performance and so limited in his responses to his general environment, that there is little possibility of and no immediate need for the retention or continuance of conscious attitudes and patterns of behavior. Instead, there is effected a dissociation from the immediate circumstances, more adequate and complete than can be achieved by suggestion in the usual process of trance induction. Hence, the performance becomes exceedingly restricted in character, occurs at a level of awareness distinct from that of ordinary waking consciousness and derives from a remote situation. In brief, it is a phenomenon of sequence,

is based upon the revivification of the hypnotic elements of another situation, and thus, is limited to hypnotic behavior.

The applicability of the above discussion to the problem of training subjects to develop more profound trances is apparent. Also, the value of repeated trance inductions to secure more profound hypnotic states is generally recognized, and this same purpose can be served more satisfactorily, readily, and easily by the utilization of the post-hypnotic performance and its concomitant trance. Especially is this so since the post-hypnotic performance provides an opportunity to secure a trance state quickly and unexpectedly without the subject having any opportunity to prepare himself or to make any special and unnecessary adjustments for his behavior. Instead, the subject suddenly finds himself in the hypnotic state and limited to patterns of response and behavior belonging only to that state. Hence, training can be accomplished without a laborious process of effecting by suggestion a dissociation of waking patterns of behavior, provided, of course, that the subject is essentially willing to forego the passive participation constituting a part of the usual training procedure.

The direct evocation of specific hypnotic phenomena without recourse to suggestion has been illustrated in the experimental account above. While the same thing may be done in the ordinary induced trance, there has been frequent and often well-founded criticism to the effect that many times the hypnotic behavior elicited was a direct response to intentional or unintentional suggestions given during the trance induction or to unexpected constructions placed by the subject upon suggestions. Behavior so elicited is expressive only of the hypnotic tendency to automatic obedience and it is not a direct expression of the hypnotic state itself. As shown in the above account, the utilization of the spontaneous post-hypnotic trance permits a direct evocation of specific phenomena without the questionable effects of a long series of suggestions given during the process of induction.

In the therapeutic situation, the utilization of the spontaneous post-hypnotic trance possesses special values for hypnotic psychotherapy, since it precludes the development of resistances and renders the patient particularly susceptible to therapeutic

suggestions. Also, the amnesia following this spontaneous trance is less easily broken down by the patient's desire to remember what suggestions have been given, as is so often the case in relation to induced trances. Hence, there is less likelihood of the patient controverting the psychotherapy given. In addition, the spontaneous post-hypnotic trance permits an easy combination of waking and hypnotic therapy, often an absolute essential for successful results. However, this problem of the combination of waking and hypnotic psychotherapy, or, more generally, the integration of hypnotic and post-hypnotic behavior with the conscious stream of activity, does not come within the scope of this paper.

THE SPONTANEOUS POST-HYPNOTIC TRANCE AND DISSOCIATION PHENOMENA

Little that is definitive can be said about the significance of the spontaneous trance in relation to both the original trance and the post-hypnotic performance as dissociation phenomena, since extensive controlled experimental work needs to be done to establish this point as well as the concept itself. However, careful observation discloses consistently that post-hypnotic behavior simply irrupts or "breaks through" into the conscious stream of activity and fails to become an integral part of that activity except as a retrospective addition. Perhaps the best illustration of this dissociated character of the trance and the post-hypnotic act may be found in the following examples: As the subject was conversing casually with others in the room, he was interrupted in the middle of a sentence by the predetermined cue for a post-hypnotic act requiring a brief absence from the room. Immediately upon perceiving the cue, the subject discontinued the remark he was making, manifested the typical post-hypnotic trance behavior, executed the act, returned to his chair, readjusted himself to his original position, seemed to go through a process of awakening, and took up his remark and continued it from the exact point of interruption. Another subject, instructed to respond instantly to a sharp auditory stimulus serving as the

cue for a post-hypnotic act, was interrupted in the middle of the pronunciation of a long word while casually conversing with others present. His performance of the post-hypnotic act was then interfered with and the subject was used for a period of 15 to 20 minutes to demonstrate to the observers present a variety of hypnotic phenomena, following which the subject was told to "*go ahead.*" In obedience to this vague suggestion, the subject proceeded to complete his performance of the post-hypnotic act, returned to his original position, re-adjusted himself, awakened, and completed the utterance of the interrupted word and continued in the same line of conversation, apparently totally unaware that there had been a lengthy interruption.

A subject similarly interrupted in the midst of rapid typing and used to demonstrate various phenomena, upon returning to his original position at the typewriter, awakened and unhesitatingly resumed his typing task without any apparent necessity to reorient himself visually. Apparently he had held his orientation to his task in complete abeyance for ready resumption. This same type of procedure, with various control measures, has been repeated many times with similar and consistent results.

Not always, however, do the subjects return after a post-hypnotic performance with such precision to the original waking train of thought. Sometimes it is picked up further along in the natural course of its development, as is shown by an interruption of the subject by post-hypnotic activity while reciting the first part of a poem and a continuation by the subject upon awakening with the recitation of the last part, with a discoverable firm belief on the part of the subject that the intervening stanzas had been recited. Some subjects, however, show marked confusion, which may be illustrated by the subject who declared, "*I've forgotten what I was just talking about,*" and required aid in renewing his remarks, but was found to believe that he had said more on the topic than was the fact. On still other occasions, subjects have manifested a hazy awareness of the post-hypnotic act and have digressed briefly to remark about some unusual circumstance apparently just discovered, as if seeking an explanation of the

peculiar change in the situation of which they had just become somewhat aware. But, on the whole, when the subject is left to re-adjust his behavior after an interpolated post-hypnotic performance without interference of any sort from the observers, there tends to be a complete amnesia for the trance and its events and an approximate return to the general situation with seemingly no awareness of any changes in it.

From these examples, typical of numerous instances, the statement is warranted that the post-hypnotic act and its spontaneously developed post-hypnotic trance constitute forms of dissociation phenomena and, hence, that they offer an opportunity to study experimentally the problem of dissociation. Similarly suggestive is the apparent continuance and independence of waking trains of thought during the trance state, despite other interpolated behavior as shown in the examples above.

Another comment that should be made before discussing the direct experimental implications, concerns the usual conditions under which these observations were made, namely, those of a general social gathering in which the topic of hypnosis was discussed with the possibility of demonstrations, but in such fashion that the subjects were unaware of any deliberate specific experimental intentions in relation to them on the part of the authors and their assistants. Maneuvering of the conversation would lead to the recitation of a poem or the giving of some famous quotations by the subject or the carrying on of guessing games, thus permitting a demonstration of the continuance of the original waking trains of thought, despite any interruption that might be occasioned by post-hypnotic acts. Our general purpose in these informal settings was the avoidance of those limitations or restrictions upon patterns of response that obtain when the subject is aware that his behavior is under direct scrutiny. In our experience, the necessity for the avoidance of overt study in hypnotic work cannot be over-emphasized. The natural course of behavior rather than the limited formalized pattern that may be expected in a strictly laboratory setting usually proves the more informative.

APPLICATIONS OF THE SPONTANEOUS POST-HYPNOTIC TRANCE IN EXPERIMENTAL WORK ON DISSOCIATION

The dissociation and independence of post-hypnotic behavior from the conscious stream of activity, the failure of integration of hypnotically motivated behavior with ordinary behavior constitute significant considerations for which there must be adequate provision in any experimental work involving both waking and post-hypnotic behavior. Hence, in studies directed to the investigation of the capacity to perform simultaneously different tasks, such as reading aloud in the waking state and doing mental addition as a post-hypnotic task, provision must be made to keep the tasks entirely independent and not contingent upon one another. While provision is easily made for the post-hypnotic activity, extreme care must be exercised to insure that the waking behavior derives entirely out of a situation belonging wholly to the waking state and that the development of a spontaneous post-hypnotic trance does not interfere significantly with the waking behavior. In Messerschmidt's experiment, mentioned previously, none of these provisions was made, which accounts for her unsatisfactory and inconclusive findings.

One needs only to observe critically a subject in such an experimental situation as Messerschmidt devised to note the constant, rapid fluctuation from one state of awareness to another of a more limited character. The unsatisfactory results obtained under such conditions are not indicative of a lack of capacity on the part of the subject, but, rather, they indicate the obstructive effects of the post-hypnotic trance developments and the interdependence of the two tasks. Accordingly, in experimental approaches to the concept of dissociation, the problem is actually one of devising a technique by which the independence of the tasks is maintained despite any simultaneity of the performances.

In brief, an adequate technique should be one that limits the post-hypnotic act to a single aspect of an entire task, of which the post-hypnotic performance represents only the initiation or culmination of the unconsciously performed activity, while the consciously performed task derives wholly from the ordinary course of events belonging entirely to the waking situation.

To illustrate this type of technique, the following examples may be cited: A farm boy subject was instructed in the trance state that thereafter for a week, every time he pumped water to fill a certain watering trough which was out of sight and hearing from the pump, and which was known by him to require 250 strokes of the pump handle to fill, he was to turn and walk to the trough the instant that it was full. Thus, the post-hypnotic act was an extremely limited part of a larger implied task, and any post-hypnotic trance manifestations would necessarily be limited to the specified post-hypnotic act.

A few days later an agreement was made in the ordinary waking state that the subject would be relieved of a certain onerous task much disliked by him if he were able to spell correctly most of the words given him by the hypnotist, the words to be selected from his own school spelling book. To this the subject agreed eagerly, and as the spelling test started, the boy's father appeared, in accord with secret arrangements, and demanded that the watering trough be filled immediately. Accordingly, the spelling test was conducted at the pump, where, as the subject pumped, one word after another was given him as rapidly as he spelled them. Suddenly, the subject interrupted his spelling, ceased pumping, and turned and walked to the trough, his behavior typical of the post-hypnotic trance state. The trough was found to be full. Repetitions of the experiment elicited the same results. Also, independent counting of the pump handle strokes disclosed the subject to be keeping accurate count despite the task of spelling. Yet, repetitions of the experiment in which the subject was instructed to count the strokes silently as the post-hypnotic task itself, while spelling aloud as a conscious task, led to unsatisfactory results, specifically, confusion of the spelling with the counting. This admixture in his performance bewildered him greatly, since, as a consequence of his amnesia for the post-hypnotic suggestions, he could not understand his frequent utterance of a number in place of a letter in his spelling.

When an attempt was made to have this subject count the strokes and spell as simultaneous waking tasks, he was found to be totally unable to do so except by deliberate purposeful pauses

and by a definite alternation of tasks. After much effort in this regard, the subject spontaneously suggested, "*I can guess the number of strokes better instead of trying to count them while I'm spelling.*" A test of this disclosed that the subject was able to "guess" accurately, but when he was questioned later in the hypnotic trance, he explained that the "guess" was only a conscious belief or understanding on his part, and that he had actually counted the strokes in the same manner as he had in the original experimental trials.

In a similar experiment, a stenographer was told in the trance state that for the next week, while taking dictation she would change pencils on the 320th word, the 550th word and the 725th word. These instructions limited the post-hypnotic act to a very small aspect of the total task. During that time she took dictation from three psychiatrists, each of whom noted the phrases at which she changed pencils. Despite the fact that she used many combined word phrases (symbols combining two or more words) it was discovered by count later that she approximated the correct number closely, never exceeding an error of 10 and averaging an error of three words.

Another important item is the fact that each time she changed pencils at the specified number of words, the subject became confused, manifested briefly the evidences of a spontaneous post-hypnotic trance and had to have a repetition of some of the dictation. Nevertheless, she could change pencils elsewhere than on the specified words without any interruption of her writing. Furthermore, her general behavior, except for the transient disturbances noted above, disclosed nothing unusual to the three psychiatrists, who, although unacquainted with the experimental situation, had been instructed to observe her behavior carefully, and to give dictation at their customary speed, which ranged between 100 and 120 words a minute. Likewise, when the hypnotist himself gave her carefully timed dictation, no unusual behavior was noted except the transient disturbances in direct relation to the specified words.

Yet, the same subject, instructed as the post-hypnotic task to count the words as they were dictated, failed completely both in

her counting and in her writing, as might be predicted if full consideration were given to habituation and learning processes and attention factors, apart from the influence of post-hypnotic trance manifestations.

An attempt was made to have her perform the two tasks as a single waking performance, but she was found unable to divide her attention sufficiently both to count correctly and to attend to the dictation. However, when it was suggested to her that she attend only to the dictation, and merely "guess" when she reached the designated number of words, it was found that she could approximate the correct count. In a subsequent hypnotic trance, she explained that the permission to "guess" permitted her to dismiss the count from her "conscious mind" so that she "could do it subconsciously."

As a control measure for the above experiments, non-hypnotic subjects and hypnotic subjects who had not been used in this type of experimentation were asked to "guess" in similar experimental situations. Their replies in all instances were found to be calculated, inaccurate approximations based upon various general considerations such as time elapsed or the number of pages covered, rather than an attempt to make an actual count.

A slightly different approach to the problem of simultaneous tasks at different levels of awareness is the utilization of post-hypnotic suggestion simply to initiate a form of behavior which then continues as an automatic activity not impinging upon the subject's conscious awareness.

To illustrate: Another stenographer was instructed in the deep trance that the appearance of the hypnotist in her office would constitute a cue for her left hand to begin automatic writing without her conscious awareness of it, and that this writing was to be discontinued immediately upon his departure. Thus, she was given post-hypnotic suggestions serving directly to initiate and to terminate a certain form of behavior. Repeatedly thereafter, whenever the hypnotist entered her office she manifested briefly the development of a post-hypnotic trance with a definite disruption of her activities, particularly so if she were engaged in typing. Under such circumstances, the post-hypnotic trance

would persist until she had been excused from one or the other of the two tasks. Care was taken, however, to enter her office frequently when she was sitting at a desk engaged in taking dictation from some one of the hypnotist's colleagues. In this situation, she would manifest a brief spontaneous post-hypnotic trance which would disrupt her immediate activity and this would be followed by a resumption of her normal dictation behavior, accompanied by a continuous automatic writing with her left hand, which would be done on the desk top, the desk blotter, or any handy sheet of paper. If no pencil were available, her hand would still go through writing movements. Upon the departure of the hypnotist from the office, there would again occur a brief spontaneous post-hypnotic trance resulting in a disruption of her normal dictation behavior and a discontinuance of the automatic writing.

On more than one occasion, one of the psychiatrists giving dictation, who had the habit of sitting with his back toward her, responded to the interruption occasioned by the spontaneous trance and her consequent request for repetition, as if it were caused by some unfamiliar medical term or by unclear enunciation on his part, and he did not become aware of the additional post-hypnotic activity. There seemed to be no interference by the automatic writing with the conscious waking performance, although the automatic writing often included phrases from the dictation as well as other sentences and phrases related to other matters.

Conversely, there seemed to be no interference by the waking activity with the automatic writing. Each was done with the same degree of facility and legibility as when either constituted the sole task for the subject.

It was also possible for the hypnotist to give dictation to this subject in the ordinary course of the daily routine, but the spontaneous post-hypnotic trance developing when he entered her office for this purpose tended to be more prolonged than was the case when his entrance merely interrupted the dictation of the other psychiatrists.

When, however, an attempt was made to have this subject

take dictation after she had been allowed to become consciously aware of the fact that her left hand was doing automatic writing, it was discovered that she could not take dictation successfully, nor could she do the automatic writing except by a process of alternating the tasks. When ample proof had been given to her that she had performed such tasks simultaneously in the past, she explained that she could probably do it if she were not asked to keep the automatic writing in mind while taking dictation, that she could take dictation adequately if she were permitted to "forget about the automatic writing."

In these three examples, the spontaneous post-hypnotic trance was limited to a minor aspect of the larger implied post-hypnotic task and hence, its interference with the concurrent conscious activity was decidedly brief in character. Also, in each instance, neither of the two tasks performed simultaneously was contingent upon the other. The waking one derived entirely out of the routine course of ordinary waking events having no relation, however remote, to the trance state in which the post-hypnotic suggestions were given. In all instances, the subjects were entirely free to engage simultaneously in two wholly independent activities without the burden of a third task of coördinating them.

Apparently, then, the essential technical consideration in the simultaneous performance of two separate and distinct tasks, each at a different level of awareness, which is not ordinarily possible at a single level of awareness, consists in the provision of some form of motivation sufficient to set into action a train of learned activity which will then continue indefinitely at one level of awareness, despite the initiation or continuation of another train of activity at another level.

CONCLUSIONS

1. A survey of the literature discloses that, although there has been frequent recognition of the fact that post-hypnotic suggestions lead to the development of a peculiar mental state in the hypnotic subject, there has been no direct study made of that special mental condition. Neither has there been provision nor

allowance made for its existence and its possible significant influences upon results obtained from post-hypnotic suggestions.

2. The significant change in the subject's mental state, in direct relation to the performance of a post-hypnotic act, has been found by extensive observation and experimentation to signify the development of a spontaneous, self-limited post-hypnotic trance, which constitutes an integral part of the process of response to and execution of post-hypnotic commands.

3. The spontaneous post-hypnotic trance may be single or multiple, brief or prolonged, but in general it appears for only a moment or two at the initiation of the post-hypnotic performance, and hence, it is easily overlooked. Its specific manifestations and residual effects form an essentially constant pattern, despite variations in the duration of the separate items of behavior caused by the purposes served and the individuality of the subjects.

4. Demonstration and testing of the spontaneous post-hypnotic trance are usually best accomplished at the moment of the initiation of the post-hypnotic performance by interference either with the subject or with the suggested act. Properly given, such interference ordinarily leads to an immediate arrest in the subject's behavior, and a prolongation of the spontaneous post-hypnotic trance, permitting a direct evocation of hypnotic phenomena typical of the ordinary induced hypnotic trance. Occasionally, however, special types of hypnotic behavior may be elicited by interference improperly given or which causes a significant alteration of the post-hypnotic situation.

5. The lapse of an indefinite period of time between the giving of a post-hypnotic suggestion and the opportunity for its execution does not affect the development of a spontaneous post-hypnotic trance as an integral part of the post-hypnotic performance.

6. Apparent exceptions to the development of the spontaneous post-hypnotic trance as an integral part of the post-hypnotic performance are found to derive from significant changes in the intended post-hypnotic situation which alter or transform it into one of another character.

7. The spontaneous post-hypnotic trance is essentially a phenomenon of sequence, since it constitutes a revivification of the hypnotic elements of the trance situation in which the specific post-hypnotic suggestion was given. Hence, its development is a criterion of the validity of the previous trance.

8. The spontaneous post-hypnotic trance may be used advantageously as a special experimental and therapeutic technique, since it obviates various of the difficulties inherent in the usual method of trance induction.

9. The post-hypnotic performance and its associated spontaneous trance constitute dissociation phenomena since they break into the ordinary stream of conscious activity as interpolations, and since they do not become integrated with the ordinary course of conscious activity.

10. Post-hypnotic suggestion may be utilized effectively to study the capacity to perform simultaneously two separate and distinct tasks, each at a different level of awareness, if adequate provision be made for the nature and character of post-hypnotic behavior.

REFERENCES

1. BERNHELM, H. *Suggestive Therapeutics*. New York: Putnam, 1895.
2. BINET, A., & FERE, C. *Animal Magnetism*. New York: Appleton, 1888.
3. BRAMWELL, J. M. *Hypnotism*. London: Rider, 1921.
4. BRICKNER, R. M., & KUBIE, L. S. A miniature psychotic storm produced by a super-ego conflict over simple post-hypnotic suggestion. *Psychoanal. Quart.*, 1936, 5, 467-487.
5. ERICKSON, M. H. A study of an experimental neurosis hypnotically induced in a case of ejaculatio praecox. *Brit. J. Med. Psychol.*, 1935, 15, 34-50.
6. HULL, C. L. *Hypnosis and Suggestibility*. New York: Appleton-Century, 1933.
7. ———. Quantitative methods of investigating hypnotic suggestion: Part I. *J. Abn. & Soc. Psychol.*, 1930, 25, 200-223; Part II. *J. Abn.*, 1931, 25, 390-417.
8. HUSTON, P. E., SHAKOW, D., & ERICKSON, M. H. A study of hypnotically induced complexes by means of the Luria technique. *J. Gen. Psychol.*, 1934, 11, 65-97.
9. LUNDHOLM, H. An experimental study of functional anesthetics as induced by suggestion in hypnosis. *J. Abn. & Soc. Psychol.*, 1928, 23, 337-355.
10. MESSERSCHMIDT, R. A quantitative investigation of the alleged independent operation of conscious and subconscious processes. *J. Abn. & Soc. Psychol.*, 1927-1928, 22, 325-340.

11. PLATONOW, K. I. On the objective proof of the experimental personality age regression. *J. Exper. Psychol.*, 1933, 9, 190-210
12. SCHILDER, P., & KAUDERS, O. Hypnosis. Washington, D C : Nerv. & Ment. Dis. Pub. Co, 1927.
13. SIDIS, B. The Psychology of Suggestions. New York. Appleton, 1898
14. WILLIAMS, G. W. The effect of hypnosis on muscular fatigue. *J. Abn. & Soc. Psychol.*, 1929, 24, 318-329.

THE EXPERIMENTAL INDUCTION OF A MULTIPLE PERSONALITY

PHILIP L. HARRIMAN

Bucknell University

That there is a tendency to underestimate the automatisms of normal persons is the burden of an important contribution by Solomon and Stein.¹ Furthermore, they add that some of these automatisms are analogous to the phenomena customarily subsumed under the topic "multiple personality." Their intent was not to induce the second personality by an experimental procedure, but to collect evidences of "definite motor reactions unaccompanied by consciousness." One of the most vexatious questions in abnormal psychology is the problem of the subliminal personality. Textbooks make passing references to the phenomena, but the discussions are chiefly on the descriptive, not the experimental, level.² Obviously, until these phenomena can be experimentally induced, the accounts will have questionable validity.

The possibilities of investigating some of these relatively obscure topics by the use of hypnotic procedures have been suggested by Erickson.³ Of course, many psychologists have pointed out the fact that experimental investigations making use of hypnotic suggestions are treacherous and that adequate controls are difficult, if not impossible, to establish. Within the limitations of the method, however, it does afford a relatively convenient

¹ Solomon, Leon M., and Stein, Gertrude. Normal Motor Automatism. *Psychological Rev.* (1896) 3:492-512.

² Consult, for example, Conklin, Edmund S. *Principles of Abnormal Psychology*; New York, Henry Holt, 1935 (ix and 437 pp.)—Chapter VII.

technique for the experimental approach to a number of topics which otherwise would vex the investigator. Since the hypnotic procedures are well-known by qualified psychologists and psychiatrists, there is the possibility of verifying conclusions and thus establishing some degree of validity by *consensus gentium*. This article sets forth a technique for the experimental induction of some phenomena commonly included in discussions of multiple personality; some typical results obtained by this procedure; and the preliminary report on the spontaneous appearance of a "secondary personality." An instructional film illustrating some of the results has been prepared,⁴ and a report on certain phases of the work will shortly appear.⁵

THE TECHNIQUE

The simplest procedure is to induce a profound trance, and then to suggest a rôle. Of course, a good hypnotic subject will act out the part. It is evident, however, that this procedure is invalid, since the behavior of the subject can be predicted from the nature of the suggestions. On the other hand, if it be true that many people once had an imaginary playmate, the subject may have had previous experience with a "secondary personality" and the hypnotic suggestions may effect a spontaneous re-arousal of this *alter ego*. Evidently, then, one approach is to induce a state of regression. The subject is carried back to a very early age, and then brought up to age four or five, the time of life when the imaginary playmate is a common phenomenon.⁶ The experimenter accounts for his presence by suggesting that he is "merely some neighbor who lives down the street." The subject is then questioned about personal identity, great care being required not to imply the existence of the imaginary companion through the nature of the inquiries.

³ Erickson, Milton H. The Problem of the Definition and Dynamic Values of Psychiatric Concepts. *Medical Record* (1938) 148:107-109 and 185-189

⁴ Harriman, Philip L. Cryptic Automatic Writing by a Multiple Personality. *The Psychological Cinema Register*; Bethlehem, Pennsylvania, 1941.

⁵ In the *Journal of Abnormal and Social Psychology*.

⁶ Jersild, Arthur T. *Child Psychology*. New York. Prentice-Hall, 1933 (462 pp.); pp. 271-275.

A second procedure is to condition the subject to respond to a stimulus which will effect a personality alteration. As Leuba has pointed out,⁷ the single presentation of the conditioned stimulus with the adequate stimulus may, in a hypnotic subject, establish the conditioning. I reverse my pencil as I say to the subject, "Watch my pencil—while you are still sound asleep. As I turn it over, a strange feeling sweeps over you. Everything now seems to be different and unfamiliar." Then the subject is questioned; or, without further instructions, a pencil is placed in the writing hand. Invariably the subject proceeds to do automatic writing, sometimes decipherable, often cryptic. Upon being awakened from the trance, the subject is unable to fill in the elisions which characteristically appear in automatic writing, or, in case the writing is cryptic, to decipher it. Presumably, the automatic writing is done by a "secondary personality."

A third procedure is to dispense with hypnosis altogether, and to induce a personality alteration through waking suggestions. In the case of persons who have previously been used as hypnotic subjects, the result can be readily attained by using the conditioned stimulus again. For example, when a pencil is unobtrusively placed in the writing hand, the subject commences to do automatic writing, meanwhile conversing with the experimenter and the witness. If the conditioned stimulus is the reversal of the pencil, then when the pencil is reversed while the subject is awake, a curious alteration of personality is induced. The subject gradually becomes abstracted and inattentive to the group. Upon being questioned or being supplied with writing materials, the "secondary personality" appears.

The fourth procedure is more subtle. James once wrote that, in his opinion, one in twenty persons is capable of doing spontaneous automatic writing.⁸ Conceivably, therefore, a few persons are capable of going into a spontaneous trance-like state. I have found two subjects who seem to go into a very deep trance without having received any verbal suggestions whatsoever, and who, in this condition, exhibit some of the phenomena subsumed

⁷ Leuba, Clarence J., in a paper read at the American Psychological Association, Pennsylvania State College, 1940.

under multiple personality. Of course, every teacher is well-acquainted with the spontaneous trance-like condition into which many students go during a lecture period. These two persons, however, exhibit behavior trends which are much more complex than the ennui of a student. When they are relaxed and engaged in conversing about trivialities, I slowly reverse the pencil. In each case a consistent "secondary personality" is thereby induced. This *alter ego* speaks in a different tone of voice, discusses topics wholly remote from the interests of the "normal self," and writes differently. Anticipating the conclusions, I hasten to add that, in my opinion, the phenomena represent an elaborate type of acting in which the rôle is determined by "unconscious" factors. The point is that these two persons went directly into another rôle without any other external stimulation than the slow reversal of the pencil.

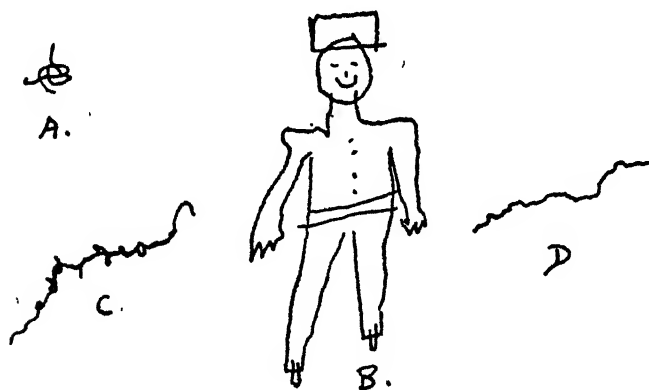
SOME TYPICAL RESULTS

Regression phenomena are evident in samples *A* and *B*. *A* represents the attempt of the subject to write his name when he had, presumably, reached the level of three years of age in a state of hypnotic regression. In responding to the request that he write his name, he broke three pencil points. Child-like, he grasped the pencil in his fist, without thumb opposition. When he was gradually brought up to the level of entrance into kindergarten, he talked volubly of *Little Danny*, who appears to have been an imaginary companion at that stage of his development. Asked about *Little Danny*, he gave a vivid description of the appearance, the prowess, and the activities of this imaginary person. No amount of innuendo and indirection, however, sufficed to induce *Danny* to put in his appearance. On the other hand, the lengthy and detailed accounts of *Danny's* personality left little doubt that the subject was genuinely reliving an episode in his life history. It may be conjectured that, had he kept up the phantasy of an imaginary companion, he might have ex-

* James, William. *Principles of Psychology*. New York: Henry Holt (1890) 1:393-398.

hibited some of the characteristics often referred to as phenomena of multiple personality.

B is an interesting example of a regression drawing completed in the state of hypnotic somnambulism. The subject, a young woman, had been taken back to the level of her earliest memory—presumably at age two and a half, and then brought up to the age at which she had just entered public school. On the Goodenough standards,⁹ this drawing is typical for about age six years, three months. As a matter of fact, this subject was enrolled in the first grade at age five years, six months. Yet, this drawing was not done by the subject herself or even by the child evoked by the hypnotic technique. If the subject's veracity is to be trusted, a *Margaret Wilson* appeared on the scene, and it was she who drew the picture. *Margaret* made her appearance spontaneously while the subject was apparently responding to the suggestions: "You are having a pleasant time with your play-things. . . . You are eagerly looking forward to the time when you will go to school. . . ."—and so on.



Later inquiry elicited the information that *Margaret Wilson* was an imaginary companion when she was of preschool age. Whereas her own hair was straight and her face covered with freckles, *Margaret* has blonde curls and a clear skin. In other

⁹ Goodenough, Florence L. *Measurement of Intelligence by Drawings*. Yonkers-on-Hudson: World Book Company, 1926 (xi and 177 pp.).

words, the imaginary companion was not only a playmate to relieve the tedium of the hours while other children were in school, but also a compensatory fiction. The subject reports that at one time in her life this imaginary companion was so real to her that she vexed her parents by her insistence that a place be set for *Margaret* at the table. She also comments that at this time of life she used to talk to herself a great deal, taking the parts of two children—herself and *Margaret*—at play. In this case, then, the suggestions appropriate to induce hypnotic regression succeeded in bringing about a spontaneous redintegration of an *alter ego*.

Specimen C is a typical example of automatic writing done in the post-hypnotic condition. A young college student had become engaged to marry, and the announcements had been made with the full approbation of the parents. A good hypnotic subject, he was placed in a deep trance, and then told that, upon observing the signal—reversal of the pencil, everything would become different. Then he was brought out of the trance by the usual method, but nothing was said regarding the restoration of his personality integration. Asked the usual questions about how he felt, he replied in a slow manner and seemed to be disoriented. He paid little attention to the experimenter or to the witnesses, but appeared to be in a condition of abstraction. Unobtrusively, I placed a pencil in his hand, and then he commenced to do some automatic writing in a cramped style. The original specimen is five-eighths of an inch long; and the remainder of his writing, which is not reproduced here, is also very minute. Examined through a reading glass, however, it is fairly legible.

The interpretation of this specimen reveals the subject's dilemma. Now that he had won the consent of the young woman and the parental blessings he had become interested in another girl, whose name he wrote linked to his. The writing goes on to mention his distress over the prospect of breaking off the engagement. Not only would he hurt the pride of his fiancée, but also he might break up the friendly relationships which had developed between their parents. Nowhere in the writing is there any suggestion for an amicable or diplomatic solution for this personality

difficulty. At the conclusion of his writing, I gave the signal—opposite reversal of the pencil—for a restoration of integration. Following a period of obvious bewilderment, he smiled at the group, and then he noticed that before him was a sheet of paper upon which there was some writing. He picked it up for closer examination and commenced to blush furiously. To spare him embarrassment over giving a public interpretation, I acted as though the paper contained nothing of any importance.

After the other members of the group had left, however, I asked the subject to read the writing. "Do I have to?" was the first response. After being assured that my interest was scientific, he read in a faltering voice, "Freddy and Jean." Now, *Jean* is not the name of his fiancée but of an attractive young woman whom he had "dated" a few times. It is significant that he no longer permits anyone to call him *Freddy*, a nickname—odious to him—that his mother formerly used. In fact, so convinced is he that *Freddy* is an objectionable, "sissy" name, that he pledged me not to reveal his present custom of dropping his first name and initial. One is tempted to conjecture, therefore, that the subject was trying to achieve a partial regression to an age level where he was not so heavily involved in a delicate personal problem; and yet, since he romantically linked *Freddy* to *Jean*, he was unwilling fully to regress.

Specimen *D* is a portion of some cryptic automatic writing done by a highly suggestible person in a waking state. While her attention was distracted by general conversations about campus affairs, I quietly placed a sheet of paper before her and put a pencil in her hand. In a few moments her hand commenced to move, apparently without her awareness; and presently the paper was covered with the usual type of cryptic writing. At length, her attention was drawn to the movements of her hand and to the writing; and she watched the action with an air of mixed confusion and interest. Spontaneously, she exclaimed, "I'm not doing that!" Since I made no comment, her hand continued to write for a while longer. Then she was requested to tell what the writing signified. "Did I do that?" she asked. Assured that no one else had done it, she still persisted in her allegations that she

had no notion of what, if any, meaning the writing had. Upon examining the paper closely, she remarked several times, "It doesn't seem as though I did that. . . . Did I write that? . . . Well, it doesn't make sense to me."

I commenced to speak of the use of x in algebra as the symbol of the unknown quantity and mentioned the desirability of using *literal number* instead of *unknown quantity* in teaching the subject in high school. Politely but restively, she endured this bit of pedantry which was introduced as a new topic of conversation. Since she was a member of my class in educational psychology, she may have assumed that the talk pertained to the morning's discussion. At length, the writer spoke of the need for using other letters of the alphabet, w for instance, instead of x . When w —"double you"—had been casually referred to, she straightened up in the chair and paid no attention to the continuance of the dry lecture. Picking up the sheet of paper upon which she had done the cryptic writing, she began to laugh. "Why, here I wrote: 'I must do more work.' That's right; now that vacation is over, I certainly must get busy on my assignments. But I really didn't know what it meant when you asked me a few minutes ago." Thus she went on to decipher the cryptic automatic writing.

One of the complex phenomena which has thus far come to my attention was the spontaneous appearance of a fully systematized *alter ego*. While a small group was discussing seminar reports in my office, I observed that a young woman was becoming somewhat drowsy. Without noticeably distracting attention from the speaker, I commenced to play with my pencil. Shifting my chair to face the bored young woman, I began to turn the pencil over slowly. In a few moments she became somewhat more alert, but looked in a quizzical manner at the other members of the seminar group. When her glance included me, she seemed to be indifferent and perfunctory, as though I were a stranger. Suspecting that a personality alteration had taken place, I then called her by name and asked what topic she would prepare. The subject, however, paid no attention to me, other than a cursory look when I first spoke. Her name was called once more, but still

she made no response. Then there ensued a rather unusual colloquy:

Q. I was addressing you.

A. Oh, excuse me; I didn't hear you mention me.

Q. What is your name, if you please?

A. Why, I'm Helen Williams.

Q. Who am I, then?

A. Your face looks familiar, but I never saw you before, so far as I can recall.

Q. Do you know these people. *Here I referred to the members of the group seated about the table.*

A. No.

Q. What do you do?

A. I work in the bank here in F—. *She mentioned the name of a small Pennsylvania city where the subject herself lives.*

Q. By the way, where are we?

A. Why, in F—, of course. I seldom get away.

Q. Have you ever been to college?

A. No, I went to work in the bank as soon as I left high school. Mary has invited me down to dances, but I have never gone.

These samples are indicative of the general tenor of a lengthy conversation. At length, *Helen* seemed to become a bit resentful that a group of apparent strangers should pose so many questions for her to answer. She spoke of her pride in being able to earn her own living and of her absence of regrets that she, unlike Mary, had no opportunity to attend college. Then she talked briefly about how college students become smug, learn to waste time, and know nothing about the satisfactions of earning their own living. After these declarations, she refused to participate in any further discussion, but she made no attempt to leave the room. I now took the initiative and directed the discussion back to the term reports. When I had indirectly informed the group of my desire to take their attention away from the "visitor," I began to play with the pencil again. At length, I commenced to turn it in the reverse direction from that in which it had first been rotated.

In a short while, Mary was once more in evidence as a drowsy, bored member of the group. Occasion was found to address a question to her, and she replied in a perfunctory man-

ner. Suddenly she was asked, "Who is Helen Williams?" "How did you know about her!" was the immediate response. "She is a girl I went to high school with, and now she is working in a bank at home. She's my best friend. When I'm home at vacations, we are inseparable." It was clear that her surprise was unfeigned and genuine. Private inquiry brought out the fact that she is somewhat envious of her friend's business success and economic independence. Frequently she identifies herself with Helen and wishes that she, too, were successful in employment. When she is bored by lecturers, she finds compensatory satisfactions in pleasant daydreams about being in her friend's place. It seems to be evident that the stimulus—reversal of the pencil—was adequate to elicit a remarkably vivid daydream on this occasion and that the rôle was completely acted out. Subsequently I have elicited *Helen* on numbers of occasions and have sustained the theory that *Helen* has a consistent personality, with a manner of speaking, writing, and behaving unlike that which is characteristic of Mary.

This subject appears to be a normal young woman of twenty-one. Her academic record has been superior, and she has participated in many campus activities, particularly in dramatics. Therein, undoubtedly, lies the explanation for her dramatic portrayal of a "secondary personality." Like some other students in a liberal arts curriculum, she feels that she will be graduated unprepared to do anything which will commend her to an employer. Consequently, she debates the wisdom of her decision to attend college for four years and to earn an A.B. degree. Now, as graduation draws near, she is especially doubtful about her choice of a life plan; and she often wishes that she had gone directly into some form of employment after high school. Her best friend has been quite successful in a bank, and she has been given three good raises in pay. No wonder, then, that she has phantasies about being in Helen's place. Habituated to take parts in dramatic club productions, she has been trained to act out her rôles. Hence, in the seminar, partly stimulated by the presence of an audience and partly dissociated by the vividness of a pleasant daydream, she enacts a part as in a play.

DISCUSSION

In describing phenomena like these, a reporter must constantly avoid the temptation to make fantastic, unwarranted interpretations. Daily life affords many illustrations of actions done without full awareness or intent. For example, a man takes off his clothing in order to get ready for a formal party; but, instead of donning his tuxedo after the bath, he puts on pajamas and goes to bed. Most readers have had the vexatious experience of mislaying the automobile keys during a period of abstraction. College students frequently decorate their notebooks with cryptic designs and phrases, the translations of which might be a difficult matter if they were insistently questioned. The frequency with which normal people engage in some of the "multiple personality" behavior is attested by the widespread use of the slang terms "pixillated" and "doodling." Perhaps some of the phenomena here described are no more unusual or significant than are the forms of behavior designated by these expressive slang words.

When the rôle for a hypnotic subject is suggested by the operator, the results seem to be unworthy of comment. As many writers have pointed out, a good subject will accept various rôles, the nature of which is directly dependent upon the suggestions. When, however, a rôle is taken without any direct suggestions by the operator, the results are more interesting. If by innuendo and indirection a rôle can be elicited, there is reason to believe that there is some basis for it in the structure of the personality of the subject. The vague suggestions may serve to effect a reintegration of well-established, habitual daydreams. When the hypnotic subject takes a rôle without any suggestions whatsoever, the results may have some real value in contributing to a knowledge of the person's daydreams and past experiences. It is evident that these "secondary personalities" strive to do something. They appear to have a goal which they strive to reach in their behavior, even though their actions and writing may superficially appear to be incoherent and purposeless.

Thus, the male subject who attempted to write his name in the regressed condition was intentful and earnest in his efforts.

When, after the awakening, he was shown the result, he displayed some curiosity and interest. At first, he did not realize that he had done it; but his reaction was nonetheless impersonal and detached. The purposeful aspect of "Margaret Wilson's" behavior was apparent. While she drew the picture of the man, she was intent upon the task. How far she was striving to "please" this investigator by her regression he is unprepared to say. The evidence is that she was entirely unaware of what she had done during the trance. When she was questioned about "Margaret Wilson," she appeared to be genuinely amazed that I knew anything about that episode in her early life. She immediately proceeded to give a complete account of many childhood experiences, speaking as though she were under a compulsion to clarify the whole matter.

The young man who wrote his name linked to that of a new "flame" was obviously laboring under some profound emotional difficulties. Just how he could escape from the situation was not at that time clear to him. On the other hand, his thoughts were pre-occupied with the new object of his affections. In the state of hypersuggestibility, therefore, he revealed the whole problem which must have been intruding into his consciousness repeatedly. He was striving to work out a solution to a difficult personal problem.

The young woman who contributed sample *D* had ample reason to be worried about her failure to meet academic obligations. Hence she wrote out her good resolution—"I must do more work." Upon being awakened from the trance, she was unable to decipher the writing; but when the signal was given, the meaning became clear. Unless one has had some experience with the interpretations of cryptic automatic writing, he might presume that the translations are nothing more than free associations elicited by the sight of the writing. A few simple investigations, however, will convince the skeptic that the subjects actually translate the cryptic material. When they write out the intelligible versions, they pause at places which seem to be obscure. Upon subsequent trials, after the experience of doing the writing has been recalled through hypnotic suggestions, they in-

variably give the same translations. If they are questioned about certain parts of the writing, they decipher it readily. Although these criteria are distressingly subjective, there seems to be little doubt as to the genuineness of the phenomena.

Occasionally one who experiments with suggestions finds a startling case, such as that of *Helen Williams*. Sometimes these persons, like Mary, are consummate actors or actresses. Then there is a grave danger lest the observer read into the case his own precommitments about the phenomena of multiple personality. There is no doubt about the fact that Mary was fully experiencing her rôle at the time and that she was intellectually honest in her protestations about not knowing of the appearance of Helen. For the time being, Helen was present—but in a fictitious sense. Mary was acting out her compensatory daydreams in a spontaneously induced trance. In subsequent experiments this rôle was quickly induced, and it became apparent that the daydreams were thoroughly systematized. The only perplexing feature of the whole event was that Mary did not know afterwards that Helen had appeared. This perplexity is removed, however, when one considers that post-hypnotic amnesia is a common phenomenon and that the subject was in a hypnotic trance induced by a slight cue. Of course, it is seldom that one finds a subject who will immediately go into a deep trance in response to a signal such as the reversal of the pencil.

The use of the *w*—"double you"—to "resynthesize" the girl's personality was a happy accident. Wondering how to bring back the personality of the subject who wrote *D*, I purposefully dramatized the technique. Those who have experimented with hypnotic phenomena are fully cognizant of the possibilities for using any type of suggestion. In fact, if the subject is put into a second trance and told that he or she now recalls fully the behavior done in the preceding trance, then the whole matter becomes clear to the subject. There is no need to go over the detailed events, since a confident suggestion is adequate to effect a redintegration of the entire experience. There is a tendency to use an elaborate procedure, and then to assume that the procedure is essential. Thus, it would appear, the phenomena are

enormously complicated. As a matter of fact, simple procedures succeed just as well.

CONCLUSIONS

This report deals with some phases of behavior which are often included within the topic *multiple personality*. As Solomon and Stein pointed out nearly a half century ago, many of these phenomena are analogous to normal automatisms. More complex actions can be experimentally induced, either through the use of hypnotic or waking suggestions. Although some of these results lend themselves to far-fetched interpretations, they may be explained much more simply as the result of the suggestions themselves. A rôle is either established by the experimenter, or conditions are established which are favorable for the subject to act out spontaneously a rôle which is already present in the makeup of his or her personality. In the exploration of a multiple personality, therefore, the investigator must take the utmost precautions to avoid suggesting a rôle and to refrain from making unwarranted interpretations of mental processes which may be present in a vast number of normal persons.

PART VII

DIFFERENTIAL PSYCHOLOGY

THE AMERICAN CASTE SYSTEM AND THE QUESTION OF NEGRO INTELLIGENCE *

HERMAN G. CANADY

West Virginia State College

I. BACKGROUND OF THE PROBLEM

The apparent subordination of Negroes to whites by limitations of their economic and social privileges is found in varying degrees in the New World. The view that the relationships between these groups in the United States (not solely in the South, but particularly there) are systematically ordered and maintained by a caste-like structure, was the creation of Warner.^{16, 17} Within either color caste is found a system of social classes, and movement up and down the ladder of class within either caste is expected; but it is not expected that there will be any movement across caste lines in either direction. Social classes overlap somewhat in their membership and participation lines are not rigidly drawn. In this respect social classes are to be contrasted with color castes, of Negroes and whites, which are mutually exclusive in their social life. Thus, it is implied that American society limits the ceiling of the Negro's social and economic ascent and sets the bounds within which the different levels of his own society will be fixed.

If it is true, therefore, that there are limitations placed upon the American Negro's participation in cultural opportunities, ob-

* This article is based on the writer's doctoral dissertation.² The research was done under the direction of Dr. A. R. Gilliland of Northwestern University, and made possible by a fellowship grant from the General Education Board.

viously he is very definitely handicapped in a comparative study of intelligence test performance, especially where whites are also concerned. This unequal share in the cultural scheme will be reflected in the Negro's responses to test questions.¹

The problem of evaluating the comparative ability of whites and Negroes by means of intelligence tests has been one of keen interest and lively controversy.² It is so infinitely complex that any direct comparison of average test scores is meaningless,¹⁰ (pp. 163-164). Involving, as it does, questions of the nature and methods of measuring these capacities, the as yet impossible task of abstracting intelligence from such pertinent factors as emotional characteristics or traits, all the unanswered problems regarding heredity and environment in ontogeny, and the need of experimental control of environmental influences, its complexity reaches fantastic proportions. But if we cannot optimistically look forward to a direct solution of such a problem, we may at least expect to see it replaced by a variety of indirect formulations which can be attacked experimentally. One of several indirect approaches to the problem will be presented in this paper.

II. OBJECTS OF THE PRESENT STUDY AND PROCEDURE

The chief problem set for this investigation was to study the relationships between social stratification and standing in intelligence tests. It was undertaken to discover whether there are significant differences in intelligence-test scores within Negro society depending upon variations in social class. We hope by this approach to do two things:

¹ It is significant that, almost without exception, all measurements of the Negro have been made with tests standardized chiefly on Northern, urban whites. Such a procedure in the light of the caste concept, is unjustifiable; for tests are applicable only to individuals similar in their experimental background to the groups upon whom they were standardized. When we administer these tests to individuals from widely different social, economic, and educational levels, we violate the basic assumption of the intelligence test method. In a recent critical survey, Garrett and Schneck⁸ write that "the examiner must always remember that comparisons are permissible only when environmental differences are absent, or at least negligible" (Pt. II, p. 24).

² For a comprehensive discussion of the techniques and results of Negro-white testing see Thompson,¹⁵ as well as several briefer reviews by Klineberg,¹⁰ Anastasi,¹ and Garth.⁵

(A) To throw further light upon the validity of the various hypotheses of Negro-white differences in intelligence scores through a study of the relation of the social class system³ to differences within the Negro group. Thus, with "race,"⁴ educational opportunity, discrimination, and other experimental variables held as constant as practicable, we shall study differential test behavior which is associated with culture and social environment. From the results it can be ascertained whether there are marked and significant differences in test standing between various groups within Negro society depending upon variations in milieu. If such differences are discovered, then Negro-white differences may be satisfactorily explained without recourse to the hypothesis of innate "racial" differences in mental ability.⁵ Standing in intelligence test might be thought of as a matter of individual differences rather than "racial" differences.

(B) To test the following hypothesis: If the subaverage test performance of the Negro is due to his position in the American social system then, other things being equal, as his socioeconomic conditions approximate more and more closely those of the white, his test scores should rise correspondingly.

Investigations of the nature proposed cannot, of course, offer

³ As defined by Warner,¹⁷ p. 19, "a class system is a series of social levels in a community, some of which are held to be superior and dominant over the subordinate and inferior ones below them"

⁴ "Race" here, and throughout this article, refers to a large group of persons possessing in common certain physical characteristics which are based upon heredity. Whenever the concept is used it will be enclosed in quotation marks to indicate that it cannot be applied except in the loosest sense. For readable accounts of many of the difficulties of "race" classification see Huxley and Haddon;⁹ also, Herskovits.⁸ Perhaps we should also stress the fact that, as concerns the American Negro, the word "race" is a sociological term. Herskovits,⁷ for example, has shown that this group constitutes a population which is "probably less than one-fourth Negro descent" (pp. 10-18), and that the average American Negro is as far removed in respect to "racial" traits from the pure Negroid type as he is from the average Causoid type. Yet old patterns of "racial" prejudice tend to persist, and those suspected of possessing the slightest strains of "Negro blood" are classified sociologically as Negroes. See also Warner.¹⁷

⁵ The problem of heredity *versus* environment, nature *versus* nurture, as it is being considered here, does not refer to individual, but to group differences. This research, therefore, is not concerned with hereditary and environmental factors which enter individual differences, but those "racial" and "non-racial" factors which have been proposed to explain group differences. See Klineberg,¹⁰ p. 154 and Anastasi.¹

a conclusive analysis of the relative contributions of cultural and biological factors. Many uncontrolled conditions still remain. Perfect control of conditions, however, cannot be obtained outside of an experimental set-up. In any event, the study of "intra-racial" differences or subgroups within the Negro "race" at least brings us a step closer to an unravelling of the complex factors which produce "racial" differences.

The study includes four hundred ninety-seven cases, comprising the entering students at West Virginia State College for the years 1935, 1936, and 1937. The freshmen represented in these classes come from twenty-seven states and the District of Columbia. The data relative to students' background and mental ability were secured from the following sources: (1) Student's application blank; (2) Sims Score Card for Socio-Economic Status (14);⁶ and, (3) scores on the American Council Psychological Examination (1935, 1936, and 1937 editions). All tests were administered by the writer and scored with the assistance of advance students in the department of psychology.

III. RESULTS AND DISCUSSION

The findings will be treated under two topics: (1) Relations between social stratification and intelligence-test scores; (2) relations between specific social factors and intelligence-test scores.

A. Relations between Social Stratification and Intelligence-test Scores

As an objective means of identifying the various social levels or classes within Negro society, Sims Score Card for Socio-Economic Status was employed. Table I shows the mean scores for the classes formed. The entire range of scores was from 4 to 35 with a mean of 16.05 and a standard deviation of 5.97. In the table this range has been divided into three parts (lower quartile,

⁶ The score of this test is offered by its author as an index of a youth's social background obtained through his answers to certain questions concerning his possessions at home—his father's occupation, and the books and other cultural indices of the family.

interquartile range, and upper quartile) indicating lower (Group I), middle (Group II), and upper (Group III) classes. The typical socio-economic conditions of members belonging to each of these classes are as follows:

- (1) Lower class (Group I).
No telephone; father a laborer, truck driver, or a miner; average number of 5.7 in family living in average of 5.29 rooms. Neither father nor mother had gone to high school. They belonged to no clubs, attended no concerts or lectures, sometimes read one magazine and owned less than twenty-five books.
- (2) Middle class (Group II).
The family usually kept one or two roomers, had a telephone, furnace heat, and over twenty-five books. The mother had gone to high school, belonged to a club which was usually a church club, and subscribed for one or two magazines. The father was mechanic, Pullman porter, janitor or miner, and had gone to high school.
- (3) Upper class (Group III).
Most frequently the father was found to be a minister, though fathers' occupations varied from miner, to doctor, dentist, lawyer, to faculty member of a university. Families of this group generally owned a car, and possessed more than 126 books. They subscribed to three or more magazines, had an average family group of 4.94 people living in an average of 7.90 rooms, attended concerts frequently, and spent their summers away from home. Both father and mother had gone to high school and one or the other to college. The youth had his own room and took private lessons in music.

Scores for socio-economic status on the Sims Score Card range from zero to a maximal of thirty-six. No one reached this maximal score which is the theoretical perfect score, but two made scores of 32 and 35, respectively. Since these scores are comparative within the group, an interpretation of the lowest and the highest is pertinent to the study. Of the three youth at the bottom of the scale (each made a score of 4) two were children of miners and one of a night watchman. There were eight persons living in three rooms. None has a telephone, furnace heat, a bathroom, or magazines. None went to a dentist except when needed, and one had never been at all. The mother went to no

lectures, clubs, or concerts; the child had had no lessons in music or dancing; and in only one case had either parent been to high school.

At the other extreme end of this scale were those scoring 32 and 35—two cases. Their fathers were a doctor and mortician. One owned two cars and the other "three or more"; both father and mother had gone to college; their families average five persons occupying ten rooms; the mother belonged to two clubs and frequently attended concerts. The youth had their own rooms, had private lessons in either or both music and dancing, had dental work done at regular intervals, and had servants in the home.

Such is the picture of the bottom and the top scores. Social Groups I, II, and III are those which will be compared as the study progresses.

1. *Gross Intelligence Scores and Social Classes.*—The mean gross test score of students in each social group is shown in Table I. It is seen that Group I is lowest with a mean score of 80.60, while Groups II and III follow with mean scores of 94.03 and 98.90, respectively.

The relationship noted above is also shown by the correlation of $.18 \pm .03$ found between intelligence scores and social status for the entire group (497 cases).

One important question is whether the difference between the means of Group I and III is significant. The standard error

TABLE I.—MEAN SOCIO-ECONOMIC SCORES AND MEAN PSYCHOLOGICAL SCORES FOR SOCIAL GROUPS I, II, AND III AND COMBINED GROUPS

Social groups	Scores for social status			Average psychological scores			No of cases
	Mean	Sigma	Range	Mean	Sigma	Range	
I	9.50	1.93	4—11	80.60	35.43	7—210	128
II	15.74	2.27	12—19	94.03	41.10	14—216	225
III	23.58	3.25	20—35	98.90	45.27	15—264	144
Total	16.05	5.97	4—35	92.12	41.13	7—264	497

of this difference is 4.90 and the critical ratio is 3.73. But what about the difference between the means of Groups I, and II, and Groups III and II? The standard error of the difference between the former (Groups I and II) is 4.17 and the critical ratio is 3.22; the difference between the latter (Groups III and II) was not

TABLE II.—MEDIAN SCORES MADE ON VARIOUS PARTS OF THE PSYCHOLOGICAL EXAMINATION ACCORDING TO SOCIAL GROUPS

Social groups	Tests					Number of cases
	Completion	Arithmetic	Artificial language	Analogies	Opposites	
(1)	(2)	(3)	(4)	(5)	(6)	(7)
I	14.00	10.48	13.71	11.36	24.58	128
II	18.35	11.57	16.35	14.18	28.09	225
III	18.96	8.60	17.37	14.80	30.63	144
Total	16.34	10.50	15.73	13.76	27.17	497

found to indicate a significant difference in intelligence test standing. It may tentatively be concluded, therefore, that there is a definite and positive relationship between the social classes within Negro society and intelligence as measured by the American Council Psychological Examination.

2. *Individual Test Scores and Social Classes.*—To further test our hypothesis on this matter, however, the examination was analyzed in order to ascertain the relative standing of the social classes in scores made on each component part of the examination. Table II gives the results of this analysis.

It can be seen that in every case except one, the arithmetic test for Group III, the groups maintain the same relative ranking as when they were compared on the basis of gross scores in Table I. Incidentally, arithmetic ability may be less influenced than most other abilities by social status.

3. *Distribution of Test Scores among Social Classes.*—A question of considerable importance from a sociological as well

as a psychological point of view is the extent to which the social classes possess students of the highest and lowest intelligent scores. Table III contains data which answer this question.

TABLE III.—PERCENTAGE OF STUDENTS REACHING OR EXCEEDING GROUP MEAN, THOSE MAKING A SCORE OF 100 OR MORE, AND THOSE MAKING LESS THAN 50 BY SOCIAL GROUP

Social groups	Number of cases	Percentage reaching or exceeding group mean	Percentage making score of 100 or more	Percentage making less than 50
(1)	(2)	(3)	(4)	(5)
I	128	35.40	25.78	21.87
II	225	51.87	41.77	15.55
III	144	52.46	40.28	10.42
Total	497	44.37	37.62	15.70

It is interesting to note that when the social classes are compared on the basis of the presence of students with high test scores and the absence of those with low ones, they maintain the same relative ranking as when they were compared on the basis of gross and individual test scores.

B. Relations between Specific Social Factors and Intelligence-test Scores

It remains now to inquire into the relationships existing between intelligence scores and certain specific social factors operating in the life of students.

1. *Parental Occupations and Intelligence Scores.*—The occupations of the fathers were secured from Sims Score Card. The method of classifying occupations suggested by Sims¹⁴ was used except in his classification of postal workers, high-school teachers, and morticians, all of whom were placed in the professional groups.⁷ Table IV shows the number of students in each occupational group and the central tendency with respect to intelligence and socio-economic status. The occupational distribution

of the four hundred ninety-seven fathers shows general agreement with that of the Negro workers of the state.¹¹

TABLE IV.—SOCIO-ECONOMIC AND INTELLIGENCE-TEST SCORES
ACCORDING TO OCCUPATIONS OF FATHERS

Occupational groups	Number of cases	Percentage of total	Median socio-economic score	Median intelligence test score	Intelligence inter-quartile range
Professional .. .	81	16.30	22.81	95.93	69.43—142.25
Commercial .. .	21	4.23	22.06	93.13	66.88—121.31
Artisan .. .	68	13.68	17.50	91.46	60.71—115.63
Skilled labor.. .	254	51.10	14.05	83.00	58.60—113.08
Unskilled labor. ---	73	14.69	13.25	79.25	56.24—107.65
Total .. .	497	100.00	14.99	81.11	59.60—117.59

A feature in Table IV of special interest is the trend of values as we run down from the professional group to the unskilled labor group. It is seen that the professional group leads when considered on the basis of both ratings for socio-economic status and intelligence with median scores of 22.81 and 95.93, respectively. The commercial group is next with median scores of 22.06 and 93.13, respectively, on the socio-economic and intelligence tests. The trend in all instances is obviously from high to low as we go down the columns. The intelligence-test standing of students as well as their socio-economic status tend to correspond to parental occupations.

2. *Intelligence-test Scores and Educational Attainments of Parents.*—One way of determining if the educational level of

¹¹ The reason for this is that the hierarchy of occupations is not the same for Negroes and whites in the United States and, consequently, a given occupation often represents a different socio-economic level in the two groups. Sims' classification of postal workers, high-school teachers, and morticians are cases in point. He places these workers in Groups II, III, and IV, respectively. The Negro postal workers, high-school teachers, and morticians certainly enjoy higher relative ranks than these workers in the professional group. See in this connection, Price,²² pp. 431-432, Canady,² pp. 16—17, 60—62, and Caliver,³ p. 66.

parents actually increases as the intelligence scores of their children increase is by ascertaining the educational attainment of parents of children whose intelligence scores fall in various quartiles. Table V furnishes information on this matter as it relates to the group under consideration. It can be seen that there is a definite relationship between the amount of schooling possessed by parents and the test standing of their children.

TABLE V.—PERCENTAGE OF STUDENTS WHOSE PARENTS ATTENDED HIGH SCHOOL OR COLLEGE, GROUPED ACCORDING TO QUARTILE IN WHICH THEIR INTELLIGENCE-TEST SCORES FALL

Quartile	No.	Attended high school		Combined, per cent	Attended college		Combined, per cent
		F, per cent	M, per cent		F, per cent	M, per cent	
Upper quartile	122	63.9	81.4	72.5	23.0	34.43	28.7
Interquartile range . .	246	53.3	64.2	58.7	20.3	29.27	24.8
Lower quartile. . .	129	45.7	46.5	46.1	10.1	17.05	13.6

3. *Intelligence-test Scores and Type of Communities.*—Although the Negro is subordinated to whites in America, caste-like restrictions are less rigid and "racial" antipathies less violent

TABLE VI.—MEAN INTELLIGENCE-TEST SCORES, GROUPED ACCORDING TO COMMUNITIES FROM WHICH STUDENTS COME

Sources of students	No of cases	Means of test score	Sigmas
Large cities in the North.	62	113.15	38.79
Large cities in the South	68	110.87	49.76
Small cities and towns in the North	41	92.62	43.50
Border states.	282	85.55	41.13
Small cities and towns in the South	52	79.00	39.57
Large cities in West Virginia	125	93.74	43.20
Small cities in West Virginia.	149	77.36	36.32

as one proceeds from the old South to the border states and on to the North,¹⁷ p. 12. Moreover, social and economic subordination are often less severe in the larger cities (population in excess of one hundred fifty thousand) of the old South than in some of the rural sections of the North. In view of these conditions, it will be interesting to ascertain the results of this study in the relation to type of communities from which students come, to scores on the intelligence test.

Table VI furnishes data which throw some light on this problem. The outstanding fact revealed by these data is the increase in mean scores made by students observed in proceeding from the southern rural areas to the northern communities. It appears, therefore, that differences in formalized relations between Negroes and whites in various sections of America are reflected in the Negroes performance on intelligence tests.

IV. SUMMARY AND CONCLUSIONS

The immediate objective of this study was to discover whether there were significant differences in intelligence scores within Negro society depending upon variation in several factors or conditions that appear to have relevance to mental development. Its second purpose is to throw further light on the various theories of Negro-white differences in ratings for intelligence. For example, if intelligence scores within the Negro group vary according to social environment, then Negro-white differences in test performance may be satisfactorily explained without recourse to the hypothesis of genetic differences in mental ability. The writer wishes to emphasize again his view that the problem of nature *versus* nurture, as it is being considered here, does not refer to individual, but to group differences.

The study included four hundred ninety-seven cases, comprising the entering students at West Virginia State College for the years 1935, 1936, and 1937. The data consisted of facts concerning students' social environment and their intelligence-test scores.

Some of the more important conclusions resulting from the study are given below:

(1) Our findings together with other data^{3, 18} furnish definite evidence that Negroes differ among themselves as individuals considerably more than they differ from whites as a group.

(2) Negro youth of the higher social levels achieve, on the average, higher intelligence scores than do those of lower social levels when these levels are determined by the Sims scale, or expressed as occupations of fathers, education attainments of parents, and communities in which youth live.

(3) The evidence presented in the present study, and other relevant data,^{1, 10} lead to the conclusion that reported Negro-white differences in intelligence test performance are due in all probability to the Negro's position in the American social system.

(4) Since disparities in social environment now are reflected in disparities in test standing, the task of American society, if it is to give effect to its democratic profession, is to work to correct the social inequalities which cause the present intellectual inequalities.⁸

V. SUGGESTIONS FOR FURTHER RESEARCH

The present study has important implications in the field of "race" differences. Among the promising lines of further investigation which has been opened by it the following would seem to be immediately feasible.⁹

(1) An investigation into the validity of test norms with special reference to Negro and white groups. One test favoring the Negro group standardized on Negro subjects. An equivalent test favoring the white group standardized on white subjects. Administer both tests to each group and determine their relative performance.

(2) Longitudinal studies in which the intellectual development of the same Negro children is followed over a period of years, or by Laswell's method of interlapping age ranges for shorter periods of time.

⁸ Our point of view here is that since social, economic, and educational opportunities are unequal in America, the low average score made by Negroes on intelligence tests is to be regarded as a disease of our society rather than evidence of group incompetence. This concept of "society as the patient," or the "sick society" has recently been analyzed by Frank.⁴

⁹ See also Witty,¹⁹ pp. 267-268.

- (3) Studies of the growth and development of Negro children in foster homes.
- (4) Comparative studies of the intelligence-test scores of Negroes living under varied environmental conditions.
- (5) Longitudinal studies of the effects of nursery-school training on successive intelligence-test ratings.
- (6) Studies of Negro-white mixture and standing in intelligence tests using improved anthropometrical measures to determine "racial" composition.
- (7) Studies of recreational interests and activities of Negro youth in different IQ groups.
- (8) Studies of the attitude of the Negro toward the Negro—color and class attitudes.
- (9) Psychogenic hazards of segregation—studies of the mental health of Negroes.
- (10) Experiments in frustration and regression in Negro children.
- (11) Comparative studies of the intelligence-test performance of urban and rural Negroes.
- (12) Genetic studies of gifted Negro children.
- (13) Studies of the socio-psychological problems of the American Negro youth.

BIBLIOGRAPHY

1. ANASTASI, ANNE. *Differential psychology; individual and group differences in behavior*. New York: Macmillan, 1937.
2. CANADY, HERMAN G. *Test standing and social setting; a comparative study of the intelligence-test scores of Negroes living under varied environment conditions*. [Northwestern University, unpublished doctor's dissertation: Evanston, Ill., 1941.]
3. CALIVER, AMBROSE. *A background study of Negro college students*. U. S. Office of Educ. Bull., No. 8, 1933
4. FRANK, L. K. "Society as the patient." *Amer. J. Sociol.*, Vol. XLIII, 1936, pp. 335-344.
5. GARTH, T. R. *Race psychology; a study of racial mental differences*. New York: McGraw-Hill, 1931.
6. GARRETT, H. E., and SCHNECK, M. R. *Psychological tests, method and results*. New York: Harper and Brothers, 1933.
7. HERSKOVITS, M. J. *Anthropometry of the American Negro*. New York: Columbia Uni. Press, 1930.
8. HERSKOVITS, M. J. *The American Negro; a study in racial crossing*. New York: Knopf, 1928.

9. HUXLEY, J S, and HADDON, A. C. *We Europeans; a survey of "racial" problems*. London: Jonathan Cape, 1935.
10. KLINEBERG, OTTO. *Race differences*. New York: Harper and Brothers, 1935
11. POSEY, T E *The Negro citizen of West Virginia*. Institute, W. Va. West Virginia State College Press, 1934
12. PRICE, J. ST. CLAIR. "Negro-white differences in general intelligence." *J. Negro Educ.*, Vol III, 1934, pp 424—452.
13. SCHWEISINGER, GLADYS C *Heredity and environment; studies in the genesis of psychological characteristics*. New York. Macmillan, 1933
14. SIMS, Y M. *The measurement of socio-economic status*. In cooperation with the Character Education Inquiry, Teachers College, Columbia University. Bloomington, Ill Public School Publishing Co, 1928
15. THOMPSON, C H. (Ed.). "The physical and mental abilities of the American Negro" *J. Negro Educ.*, Yearbook Number III, July, 1934
16. WARNER, W. L. "American caste and class." *Amer. J. Sociol.*, Vol XLII, 1936, pp. 234-237.
17. WARNER, W L., JUNKER, B H and ADAMS, W. A. *Color and human nature; Negro personality development in a northern city*. Prepared for The American Youth Commission. Washington, D C American Council on Education, 1941
18. WITTY, P. A "Research upon the American Negro" [In] *The Thirty-Ninth Yearbook of the National Society for the Study of Education*, Part I, Bloomington, Ill : Public School Publishing Co., 1940, pp 261-269.

THE EFFECT OF MENTAL DISORDER ON INTELLIGENCE *

ANNE ROE AND DAVID SHAKOW

INTRODUCTION

The use of psychometric methods in connection with clinical and research work in mental disorders, has been greatly hampered because there have been insufficient data to permit interpretation of the results. In dealing with abnormal persons, a number of questions arise which cannot be answered by the data accumulated on children, on whom most tests have been standardized, or even by data on normal adults when these are available. But these problems must be solved, at least in a general way, if test results are to be meaningful in the individual case.

In spite of the many and legitimate objections which can be raised with respect to the Stanford-Binet in use with adults, it has generally been the test of choice in the clinical situation, because of the variety of tasks involved and the insight which it can afford to a trained examiner. It is particularly the trained and sensitive examiner, however, who has most appreciated the need for data which would make it possible to place an individual with regard to his own psychiatric classification as well as with regard to normal standards. Further, there have been few data on the relative significance of the individual items as indicators of departure from normality.

* The full technical report of the work on which this paper is based will be found in Roe and Shakow, *Intelligence in Mental Disorder*, Annals of the New York Academy of Sciences, XLII, 1942, 361-490

In an attempt to find an answer to some of these problems, at least in terms of a first approximation, we embarked some years ago upon a study, concerned with groups rather than with individuals, the results of which are reported here. Our research includes an attempt to determine (a) the general test level of various diagnostic groups, and (b) the relative performance of different groups on different types of tests (profiles). The latter type of analysis leads to the very fundamental question of whether there is any relation between functional change and type of disorder. It is important to investigate fully the possibility of trends of cognitive change in any diagnostic group before the performance of an individual member of the group can be evaluated. If trends exist and can be discovered, the next logical step is the investigation of the significance of the presence or absence of conformity of the individual patient to the group.

Once the performance in various groups has been established, it is possible to compare them with a normal group and with each other, as we do here. These comparisons are valid in so far as there is no selective factor of importance other than that of the diagnosis; the groups are relatively equivalent so far as any other factors affecting mental level are concerned. For all practical purposes it is necessary to demonstrate only that the groups are drawn from the same general population, *i.e.*, the factors of age, education and social level must be considered; the age distributions in the different psychoses must be kept in mind, also, as well as the varying effects of age on educational level.

A consideration of great importance, and one frequently disregarded, is whether or not all tests of psychotics can legitimately be grouped together, even within one diagnostic classification. When the problem at hand is simply the determination of what such a group, taken as a whole, does on examination, it is valid to use any results that can be obtained. But where the objective is the comparison of various groups the situation is somewhat different.

One of the first requisites in setting up any problem is a careful specification of the population. One is faced immediately with the problem of whether to separate examinations in which

the patient was sufficiently accessible and cooperative to give results which could be accepted as a reasonably accurate picture of his functional level at the general period of the test (representative), from examinations which did not seem to do justice to the subject's ability (non-representative).

Scores are affected by cooperation to an unknown extent; but cooperation is itself often related to the severity of the psychosis. If we include all cases, we have in many of the psychotic subjects a factor not present in any of the normals. If we exclude the non-cooperative, we exclude a considerable number of the severest cases, and hence somewhat prejudice our results in favor of the psychotics. Some preliminary investigation of the problem of representativeness was thus indicated as necessary.

In comparing the profiles of two diagnostic groups, recourse is sometimes had to matching for intellectual level. Any differences found in specific performances are then considered a direct result of the disease process and not a reflection of the general lowering. This procedure although valuable for certain purposes is, however, open to serious objection. Though we know as yet comparatively little about the intra-individual relationship of different functions, it is fairly clear that the inter-correlation of functions is not the same at all levels of general intelligence. In most instances of psychosis we do find changes in cognitive functions, generally a lowering. If, then, we are comparing the psychotic and non-psychotic, either we must take only those psychotics who do not show a lowering or whose original level was so high that deterioration does not bring them below average or we must compare psychotics and dull or feeble-minded non-psychotics. In each instance we have a different selective factor in the two groups. It is, of course, entirely legitimate and illuminating to compare the feeble-minded with psychotics in this fashion, but we cannot argue from this to any conclusions with respect to normal-psychotic comparisons. Comparing matched pairs from two psychotic groups is subject to the same criticism which can be applied to psychotic-normal comparisons—different types of psychosis may affect general intellectual functioning to different degrees, and we are in danger of com-

TABLE I.—NUMBER OF CASES IN THE DIAGNOSTIC GROUPS

	Representative (R) N.	Non-representative (n-R) N.
General Paresis	35	24
Chronic Alcoholism with Psychosis	30	12
Chronic Alcoholism without psychosis	12	2
Acute Alcoholic Psychosis	17	6
Feeble-mindedness with Psychosis	60	25
Feeble-mindedness without Psychosis	27	10
Paranoid D.P.	58	31
Hebephrenic D.P.	32	29
Catatonic D.P.	30	28
Simple D.P.	22	7
Unclassified D.P.	38	25
Manic-Depressive	19	18
Psychopathic Personality with Psychosis	22	14
Psychopathic Personality without Psychosis	21	4
Paranoid Condition	22	9
Psychoneurosis	36	12
Without Psychosis	72	18
	<hr/> 553	<hr/> 274

paring, for instance, the upper third of one diagnostic group with the middle or lower third of another.

II. DESCRIPTION OF THE SAMPLE

Our material consists of the results of Stanford-Binet (1916) examinations given to 827 patients at the Worcester State Hospital during the years 1929 to 1933. The diagnostic groups and the number of patients in each are shown in Table I.

Each diagnostic group was subdivided into two groups according to whether the Stanford-Binet was considered representative or not. Comparisons between diagnostic groups and between

these and normal subjects are made with representative groups. The non-representative groups are used only for comparisons with representative groups of the same diagnostic classifications and only when they are of sufficient size for useful statistical comparison.

On the whole, the patients studied are a reasonably good sample of the total patient population in the various diagnostic groups.

III. REPRESENTATIVE AND NON-REPRESENTATIVE GROUPS

We present first the results of the comparison of representative and non-representative groups in the same diagnostic class. Some of the factors affecting representativeness are under the partial control of the subject, *viz.*, effort, interest and self-confidence. The others, almost entirely outside the subject's control, are of a relatively *temporary* kind: a psychotic episode (temporary manic, excited, or hallucinatory conditions), an emotional upset, physical handicap (loss of sensory aids such as glasses, etc.), passing physical illness (headache, etc.), fatigue, poor test conditions.

The complex of effort, interest and self-confidence are summed up in the general term of co-operativeness and are given a letter rating from A to E*; to be representative a rating of A or B must be obtained. The influence of the other factors, except "psychotic episode", is not hard to evaluate.

*A. The essential feature in the attitude designated by an A rating is that the patient must show an active interest in the tests themselves. Corollaries to this are that he should exert maximum effort in doing the tests and show interest in the results.

B. For this rating, the patient must give active and willing cooperation, and real effort, not because of a primary interest in the tests themselves, but because of some other factor, such as general friendliness or a desire to please the examiner.

C. Patients given this rating are docile and submissive but show no real interest; effort is perfunctory or spasmodic. Some urging is usually needed.

D. This rating is given to the patient who considers the examination a disagreeable task or an affront. He may be resentful, resistive, or surly. It is only after considerable urging and repeated questioning that and results are obtained.

E. Absolute refusal to cooperate results in this rating.

It is obviously difficult to define and to set arbitrary limits for a psychotic episode and to distinguish between it and a psychotic state (a relatively permanent disturbed condition). What is necessary is for the examiner to have in mind the spirit and the purpose of making these judgments. Their primary purpose is to evaluate fairly the person's intellectual *capacity*. At the same time it is necessary to determine his functioning intelligence, *i.e.*, what he has to work with at the present time. If his performance today is, from all the evidence available with regard to his psychotic condition, the best he is likely to be able to achieve during the approximate period of the next few weeks, then in this respect the examination is representative. If the examiner, however, has any basis for feeling that the patient within a few days or a week or so would achieve a higher level of performance, he must consider the examination unrepresentative. As has been stated, patients with permanent physical or serious language handicaps have been eliminated.

Studies from our laboratory in the psychomotor sphere have brought out clearly that the attitude of the subject may profoundly influence his performance. We do not know of any searching investigation of this problem with regard to psychometric performance. It would seem likely that test performance would be affected, although it has been argued that psychometric results are not fundamentally affected by attitude. The question of how much or in what way is one which can be settled only by analysis of test results under varying conditions of cooperation on the subject's part. One needs to find out not only the degree of effect of attitude but also its selectivity, if any, on different types of test items. Too, it may be different with different types of psychosis. Such an analysis also makes possible a comparison of the differences of the effect of attitude and of that of psychosis (in the one case, representative vs. non-representative results within a psychosis, and in the other, representative results in a psychosis vs. normal results) on test performance.

The non-representative sub-groups in the following diagnoses, were used in the comparison: general paresis, paranoid dementia praecox, hebephrenic dementia praecox, catatonic demen-

tia praecox, unclassified dementia praecox, manic-depressive, and psychopathic personality with psychosis.

In four of these groups (general paresis, and paranoid, hebephrenic and catatonic dementia praecox) mean scores on the Stanford-Binet were significantly lower for the non-representative group. None of the mean scores for vocabulary were significantly lower, but on digits forwards, the unrepresentative catatonic and hebephrenic dementia praecox and the manic-depressive were lower, and on digits backwards the unrepresentative hebephrenic.

Where differences existed on the individual items, the non-representative groups were always lower. Significant differences were found most frequently for items involving conceptual and associative thinking, and least frequently for memory items.

The greatest number of differences were found between the representative and non-representative subgroups of the hebephrenic dementia praecox group; very few were found in the case of the psychopathic personality with psychosis, the unclassified dementia praecox and the manic-depressive groups.

It seems reasonable to conclude, that for the study of the effect of psychosis on psychometric performance the distinction between representative and non-representative examinations should be made. Although tests actually representative may be adjudged non-representative the reverse is less likely to be true. The question of whether the exclusion of occasional representative test material affects the randomness of the sampling in a group of any size is, of course, an important one. We think that study of our distributions is an adequate answer to this query; the overlapping is too extensive to allow for any great error here.

IV. REPRESENTATIVE HOSPITAL GROUPS AND A NORMAL GROUP

We are now ready to discuss the comparisons among the various diagnostic groups and between these groups and the normal group reported by Weisenburg, Roe and McBride.*

Because of factors inherent in the nature of a particular disease and the stage at which hospitalization becomes necessary,

* Weisenburg, T., Roe, A. and McBride, K. E. *Adult Intelligence*. New York: Commonwealth Fund, 1936.

MEAN LEVELS OF VARIOUS DIAGNOSTIC GROUPS AND NORMAL
CONTROLS ON THE STANFORD-BINET

Diagnosis	N	C A	Educ (Yrs)	M.A.	Vocab.
Psychoneurosis	36	28 ⁺	9.4 ¹	14-2	54
Manic-Depressive	19	35	8.8	13-10	59
D.P. — Catatonic	30	25*	9.9*	13-9	57
Paranoid Condition	22	40	9.1	13-9	59
Normal	70	36	8.1	13-8	55
Psychopath Pers. s Psych	21	25*	7.8	13-0	49
Psychopath Pers. c Psych.	22	24*	7.6	13-0	49
D.P. — Simple	22	31	9.0	13-0	53
Chron. Alco. s Psych.	12	42	7.2	12-11	55
D.P. — Paranoid	58	34	8.5	12-11	56
Acute Alcoholic	17	36	8.7	12-10	55
Without Psychosis	72	26*	7.7	12-9*	48*
Dementia Praecox	180	30*	8.8*	12-6*	52
Chron. Alco. c Psych.	30	44*	6.9	12-2*	50
D.P. — Unclassified	38	28 ⁺	8.4	12-0*	50
General Paresis	35	41*	8.6	11-0*	47*
D.P. — Hebephrenic	32	29*	8.5	10-9*	42*
Feeble-Minded s Psych.	27	26*	5.8*	8-8*	26*
Feeble-Minded c Psych.	60	32	4.8*	7-10*	23*

* P = .05

it happens that many of these smaller groups show significant differences from the normal in mean age: the group of chronic alcoholics with psychosis and the parietic group are significantly older; the total dementia praecox group and hebephrenic, catatonic and unclassified groups are significantly younger as are also the two groups of psychopathic personality, the psychoneurotic and the without psychosis groups. In all cases, however, the groups are, by any standards, adult, and distributions overlap to a great extent. We must remember, however, in considering the older groups, the possible effect of age in some of the individuals. It is impossible to estimate this exactly, especially since for the individual Stanford-Binet items age curves are not available. Within this range of ages we may at least discount any effect of age on vocabulary.

With the younger groups the situation is to some extent reversed; where there is any effect of age on test performance it is chiefly the normals who are penalized, so that we may be reasonably certain that any differences found are in spite of, not because of, age differences.

The catatonic and the psychoneurotic groups are better educated than the normal. All others have mean years of education closely approximating the normal.

For the most part, differences in occupational distribution appear logically to reflect age differences rather than great differences in social background. The younger groups tend to have proportionately fewer professional workers and more semi-skilled and unskilled laborers, because many of these had insufficient time to learn a trade or a profession before hospitalization.

Although the changes found follow much the same pattern in all of the disordered groups, there are some differences between the various diagnoses.

Next to the hebephrenic dementia praecox, general paresis is the most severely affected of any group studied, as shown in mean and item differences from the normal. The parietic's impairment is marked—he falls at about an 11-year mental level—and general, but in addition he shows particularly severe difficulty with immediate memory and conceptual thinking.

The paretic is much more profoundly disturbed in cognitive functioning than the psychotic alcoholic, and this disturbance is very nearly as extensive as that in the hebephrenic from which it does not differ in any meaningful way. But, profound as this disturbance is, it does not reduce the paretic to the level of the feeble-minded, whether psychotic or not. In the latter comparison it is interesting to note that the paretics were consistently *relatively* better on old learned items, and the feeble-minded on immediate memory and, to a lesser extent, on conceptual thinking.

Comparisons with normals, and comparisons among the three alcoholic groups (*chronic alcoholism with psychosis, chronic alcoholism without psychosis, acute alcoholic psychosis*) bring out few statistically significant differences, but show a consistent trend. The acute and the non-psychotic chronic groups, although generally lower, differ comparatively little from the normal and from each other on test results; the psychotic chronic alcoholics tend to fall below the two others throughout and are well below normal. It would seem that psychosis rather than alcoholism, even over long periods, is the factor affecting test performance, and further that it is not alcoholism, *per se*, which induces the psychosis.

In comparison with the normal, what seems to stand out in the various types of *dementia praecox* is the closeness of the catatonic, simple and paranoid groups (in that order) to the normal. Except for a few items on which significant differences are found, there is little to distinguish these groups from the normal. But the unclassified and especially the hebephrenic are clearly distinguishable from the normal, the latter, in fact, the most definitely of any of the groups studied. The unclassified with a mental level of about 125 months are third highest in number of differences in items from the normal. These items fall largely in the conceptual and associative thinking groups. All means, except that for vocabulary, are significantly lower. The hebephrenic *dementia praecox* which differs most from normal shows this difference in all means and in all types of items. The average mental age level is somewhat below 11. Apparently psychosis has the most profound effect on this group.

A careful intercomparison of the various dementia praecox groups indicates that there is no justification for lumping all types into a total dementia praecox group, at least for psychometric research, since the differences among the types are in many instances greater than between the dementia praecoxes and other groups. In general, the catatonics are the highest group, although the simple surpass them in means for digits forwards and backwards. The paranoid and simple are very close and not far below the catatonic; the hebephrenic and unclassified definitely inferior, with the hebephrenic outstandingly low. Where many differences were found for any item, it was usually the case that the others surpassed the hebephrenics.

In the comparisons made between the various types of dementia praecox and other psychoses, outstanding findings are: little difference between paranoid dementia praecox and paranoid condition, although the latter are closer to the normal; a striking similarity between the hebephrenic and parietic groups sufficient to make the distinction of the psychometric picture all but impossible; and another striking similarity between the catatonic and manic-depressive, which makes these psychometrically indistinguishable.

The *manic-depressive* is a heterogeneous group comprising all types of patients but all of them examined during comparatively quiescent periods. The group is so like the normal in all test results—distribution, means, and items—as to be distinguishable in psychometric performance.

The groups *psychopathic personality with psychosis* and *psychopathic personality without psychosis* are extraordinarily close in age, education and mean test results, and approximate the normal, although they fall somewhat below it.

The group *paranoid condition* falls quite close to the normal and may be considered as essentially the same psychometrically.

The mean test results for *psychoneurosis* are very close to the normal, but this group tended to be somewhat higher than the normal on some individual items. We did not find our neurotics more variable than normals. The psychoneurotic group is

of a higher level than the group psychopathic personality without psychosis.

Analysis of the group diagnosed as *without psychosis* is of particular interest because of the frequent appearance in the literature of similar groups which have been used as normal controls. This group had the fourth largest number of significant differences from the normal in the item analysis. Mean test results were all markedly lower, the differences being significant for vocabulary, digits forwards and for Stanford-Binet score. The mental level of this group is 152 months. The item analysis showed the disturbance to be chiefly apparent in memory items, including vocabulary, and to a much smaller degree in thinking items.

V. RELATIONSHIP BETWEEN INTELLIGENCE & MENTAL DISORDER

In general, this study indicates that the severer the psychotic condition, the farther from the normal is the psychometric performance. Such a difference can be interpreted in two ways: (1) As demonstrating the effect of psychosis on cognitive function, or (2) As demonstrating the selective effect of intelligence on psychosis, *i.e.*, that certain types of psychoses appear at certain levels of intelligence rather than at others.

Ordinarily the latter theory is put forward, not with respect to all psychoses, but rather to particular ones, *e.g.*, the theories that an initial low level conduces to neuroticism; that feeble-mindedness is an etiological factor in manic-depressive insanity and that paranoia attacks chiefly those of superior intellectual level. The implications of this point of view have not been very carefully examined by its adherents. The fact of the existence of differences in intelligence between normal and abnormal groups does not in itself offer any clues as to the nature of the relationship; this must be inferred on other criteria and in accord with psychiatric as well as with psychological evidence. Now, if it were the case that the level of pre-psychotic intelligence in the individual determined the nature of the psychosis to which he might be subject, then the generally accepted concept of a psychosis as primarily a disorder of the personality, particularly of its affective aspects, must be in error. For such a drastic revision

of basic psychiatric theory there is no valid evidence. On the other hand, the interpretation of these differences as the effects, in the cognitive field, of the disorder of the personality is in complete accord with psychiatric knowledge, and in no way opposed by the psychological evidence.

The differences in mean test performances found by many experimenters (although not always in the same direction nor to the same extent) are not all of the evidence available. It is enlightening to compare the distributions. Such a comparison shows considerable overlapping of both ranges and standard deviations, even when the means differ significantly. This seems to us a potent argument against the interpretation that original level of intelligence may determine in any substantial part the type of psychosis.

These data together with data derived from a study of the educational level of the patients seem reasonable evidence that there are very few, if any, disorders in which the type of psychosis developed is dependent to any extent on intellectual level.

Some further comment should be made about the commonly held position that paranoid conditions of one kind or another occur more frequently in the upper levels of intelligence. It is true that our paranoid condition and paranoid dementia praecox groups are well up generally as far as means go, but again the dispersal is extensive, and if this disease has originally attacked only the more intelligent, then the deterioration in some of these cases must be much greater than the clinical picture would lead one to infer. (It is generally accepted that these types deteriorate rather slowly.) We suspect that the typical paranoid verbosity has been a confusing factor, and from the extent and press of verbal activity has been deduced a level of verbal intelligence, even of general intelligence, which is not actually present.

In the presence of any psychotic or psychopathic process, certain changes in mental functioning may occur. These are in the direction of a lowering of the level of functioning and are both general and specific in nature. There is no doubt but that some part of this lowering may be attributed to difficulties in attention, and to what extent this basic function is affected we

cannot say. It is clear, however, that it is not the only function that is affected.

Our analysis has shown that in the severely disordered mind, all types of intellectual activity are interfered with, some more and some less than others. Vocabulary is less affected in general than the other tests in all of our groups, including the normal. Vocabulary expressed in terms of MA was higher than Stanford-Binet MA, and in many of the groups the difference was considerable. To that extent, it is probably a better indicator of general level than is Stanford-Binet MA, but that it is not an adequate one is shown by the fact that in two groups it was far below the level of previous education.

One of our most striking results was the consistency with which the repetition of digits forwards was affected by psychosis. Of our 15 representative groups, 9 had means significantly lower than the normal mean. In general, the severer the clinical manifestations of the disorder the more marked the impairment in this test. We do not know how much of this disturbance is purely a memory disturbance and how much is due to disturbance of the span of attention. But that such a disturbance is present and to such an extent is a fact of importance in assessing test results in disordered groups. Repetition of digits backwards does not show so much effect. It is a task more closely related to general intellectual functioning than to simple rote memory.

As far as the type of items most affected is concerned, our analysis has shown that those primarily affected are concerned with conceptual thinking and immediate memory and that those least affected are old learning items.

Variability of performance appears to be greater in the psychotic than in the normal group, and this increase in variability is most apparent in the various dementia praecox groups.

Our material offers no evidence on the permanence of the changes which have appeared. The general improvement in mental functioning in various psychoses under the newer treatments raises this question again. It does, however, seem to be the case that in patients whose psychosis is quite severe the levels of cognitive functioning are permanently lowered.

THE ARMY PERSONNEL CLASSIFICATION SYSTEM

WALTER V. BINGHAM

The Adjutant General's Office, War Department

The personnel classification system of the United States Army, created during the first World War, has been streamlined during the past two years and adapted to the requirements of a highly complex, mobile, mechanized Army.¹

Two main objectives of classification in a fast expanding military force are to conserve man power and to expedite training.² In order to accomplish these missions, the personnel system is designed to facilitate correct initial placement of officers and soldiers, to maintain cumulative records of their subsequent experience and progress, and to provide a way of locating quickly at any time those who can do what has to be done at once—men able to undertake emergency duties or instantly to replace incapacitated members of combat teams.³ Each of these objectives, as we shall see has helped to shape army personnel practice.

¹ Anonymous. "The Army Personnel System," *Army and Navy Journal*, LXXVIII, No. 41 (June 14, 1941), 1149 ff.

Committee on Classification of Personnel in the Army, *The Personnel System of the U. S. Army*, Vol. I: *History of the Personnel System*, pp. viii + 713; Vol. II: *The Personnel Manual*, pp. viii + 342; Washington: Superintendent of Public Documents, 1919.

² Robert M. Yerkes. "Manpower and Military Effectiveness: The Case for Human Engineering," *Journal of Consulting Psychology*, 1941, 5, 205-209.

³ Walter V. Bingham. "Psychological Services in the Army," *Journal of Consulting Psychology*, 1941, 5, 221-24.

Paul Van Riper, Jr. "Personnel Administration in the United States Army," *Public Personnel Review*, 1941, 2, 199-210.

Yet another purpose of a sound classification program is to enhance morale. Officers and men are proud to belong to an organization in which each one is called upon to do what he is most fully qualified to do. What sort of team spirit could a commander expect to develop if he had to make his assignments by rote? Only when each member of a team plays a part that is within his scope and that nevertheless requires the best that is in him, will the organization develop *esprit de corps*; and that is another name for army morale.

NATURE AND MAGNITUDE OF THE TASK

The Army's problem of classification and assignment is not an easy one. Consider the vast population involved. At the end of October 1940, only 34,405 officers were on active duty, and 483,218 enlisted men; but these figures soon tripled. Within a year upwards of 80,000 more officers and a million more men had answered the call.

What a variety of abilities, aptitudes, and occupational skills these citizen soldiers brought with them! Coming from the farms, the stores, the workshops of the entire Nation, thousands of them knew how to drive a tractor or repair an ignition system. Others could expertly weld castings, repair watches, read transits, keep accounts, carve carcasses of beef, develop photographs, or perform some other necessary duty. The range of skills thus made available is seen in the *Dictionary of Occupational Titles*,⁴ which defines the names of no fewer than seventeen thousand different civilian jobs, occupations, and professions. Classification officers responsible for identifying the kinds of work performed by the soldiers in civilian life have found this dictionary an invaluable work of reference.

A surprising number of these occupational skills are needed in the American Army, but men can seldom practice their civilian callings in the military service without first learning a good deal that is new to them about the analogous military occupations.

⁴United States Employment Service. *Dictionary of Occupational Titles*, Part I: *Definitions of Titles*, pp. xxxii + 1287; Part II: *Titles and Codes*, pp. xxvi + 330; Washington: U. S. Government Printing Office, 1939.

Expert telegraphers, radio repairmen, telephone linemen and switchboard installers, for instance, need weeks if not months of army experience in preparation for duty in communications units of the Signal Corps. Nevertheless, the organization and training of such teams goes forward much faster when they are made up of men whose knowledge and experience give them a head start toward mastery of the army jobs.

To be sure, the military duty does now and then duplicate almost exactly some civilian trade. In a printing plant of the Infantry at Fort Benning, for example, where manuals, tactical exercises, and instructional problems are turned out daily by the thousands, there are infantrymen photolithing maps, operating linotype machines, and tending great cylinder presses identical in kind with those on which they had had experience in Philadelphia or New York before induction. It is amazing how many different kinds of expertness are required in each of the combat arms as well as in the auxiliary services.

The index of army occupations begins with "able seaman." Yes, experienced sailors are wanted in certain harbor defense units of the Coast Artillery Corps. At the other end of the alphabet are "X-ray technician" and "yardmaster." Occupational analysts studying the work done by our soldiers have already identified upwards of two thousand military specialties, and the list is not yet complete. Classification and assignment officers have had a great deal to learn about specific duties and qualifications in both civilian and military occupations.

The task is further complicated by the fact that the occupational supply seldom exactly matches the demand. Many more young lawyers have turned up among the selectees than the Judge Advocate General and the Quartermaster General need as lawyers. The Surgeon General, on the other hand, is confronted with a shortage of qualified medical assistants. The occurrence rates of carpenters and of painters have exceeded the requirement rates, while good cooks, general mechanics, and radio operators are at a premium. Among the other trades in which the army demand has been far in excess of the available supply are rigger, construction foreman, truckmaster, and surveyor (instru-

ment man). Such a jigsaw puzzle cannot be solved, of course, by arbitrarily forcing the pieces into place. There have to be adjustments. A young attorney, if he has the personality and the stamina of a natural leader, may fit neatly into a place of responsibility in the training of combat troops and advance to a position of command; or perhaps he may after all be only of the caliber for duty as a battery clerk.

Competence in a civilian occupation obviously is but one of the factors to be taken into the reckoning when matching men and jobs. The individual's physique, energy, intelligence, adaptability, interests, initiative, stability, and other personal characteristics on the one hand, and on the other, the needs of the service, must all be weighed if man-power is to be conserved, training facilitated, and *esprit de corps* insured. The following section tells how the Army has approached this complicated task of classifying and allocating its personnel.

PROGRESSIVE STEPS IN CLASSIFICATION

A soldier's classification is not accomplished once and for all. It is a long process, coextensive with his military career. He lives and learns, advancing in grade or reverting to a less responsible status after failure to measure up. Each significant change must be ascertained and recorded if his qualification card is truly to reflect his abilities. Of immediate interest, however, are the early stages of classification, during which an inventory is made of the assets which the citizen soldier brings with him and places at the Army's disposal. Let us follow him through four of these stages. We shall find him under scrutiny for a few hours at an Army Induction Station, for three or four days in a Reception Center, for several weeks in a Replacement Training Center, and finally in a tactical unit or other installation in which he is likely to serve for some months at least.

Induction

First, at an Army Induction Station, before the registrant or recruit actually becomes an enlisted man, a board of medical specialists thoroughly examines and assays his physique. It is

the responsibility of this board to identify at this time any who are not really fit for military service. If any disqualifying disability is discovered which may not have been apparent or did not exist at the time of an earlier examination, the man is at once given transportation home. He carries a report which may be discussed with his family physician in order that he may take steps to correct the condition or to hold it in check.

In time of war, even more than in days of peace, the Army must not be burdened with the task of attempting to train and to supervise the mentally ill or the very stupid. Those also who are bright enough but are wholly illiterate present a problem, since parts of the training program which are based on study of army manuals or other printed instructions are denied to them. And so when there is doubt as to whether a recruit has learned to read at least as well as the average man who has had the benefit of four years of schooling, he is given a short self-administering Minimum Literacy Test. This test is not unlike, but somewhat easier than, those used in New York and certain other states for ascertaining whether citizens can read well enough to be allowed to vote.

Most of the examining boards at induction stations have performed their difficult tasks superbly. The local boards, too, no matter how heavily burdened, have on the whole been careful not to select registrants who are obviously defective. Rarely have they chosen to send the community trouble-maker or the town fool. Even so, some slip-ups have occurred. Registrants have actually got through induction stations and into the Army with only one lung, one eye, club feet, an infectious illness, a criminal record, or a term in an institution for the feeble-minded. Most of these have had to be discharged and returned to their homes at considerable expense of time and money. Fortunately, the number of such mistakes in selection has been small in view of the enormous populations involved.

When the physicians at the Induction Station complete their examinations and certain necessary papers have been made out, the new recruits are solemnly sworn in and proceed to the nearest Reception Center.

Reception

There are thirty-six Reception Centers within the continental limits of the United States. Here newly inducted men are quarantined for only a few days while they are outfitted with uniforms and other clothing, inoculated, and given their first instruction in Army regulations, sanitation, and the Articles of War. Here also the qualifications of each man are ascertained in a way to be of assistance in placing him to the best advantage.

The new soldiers are interviewed by men who have been specially trained to get the facts about a recruit's background and experience. Information as to his age, schooling, occupational history, earnings, skills, athletic activities, avocations, and preferences is drawn out. Most valuable are any data the interviewer can uncover evidencing ability to lead. The men also take a General Classification Test which helps to identify promptly the fast learners and the slow.

All this information is recorded on a Qualification Card which then accompanies the soldier throughout his career in the Army. The face of this 8 by 10½ inch card is shown in Figure 1. On the reverse are the facts about physical classification, previous military experience if any, and the recommended assignment. There is space for a cumulative record of successive assignments and duties. Here also entries are to be made on successful completion of training in military specialties. The essential data are coded and punched in the margins of the card so that men with desired combinations of qualifications can be speedily located.

All this information is taken into account by the Classification Officer, who makes a tentative decision as to the man's best usefulness, and by the Assignment Officer, who sees that he goes to a training center of the particular arm or service where he is most needed.

Training

There are twenty-eight training stations known as Replacement Training Centers. They differ in capacity, the largest accommodating seventeen thousand men. During his early weeks at one of these centers, while the recruit is strengthening his

physique, getting acquainted with army ways and learning the skills which every soldier needs whatever his subsequent duties, there is a chance to observe him while performing several kinds of work, and if necessary, to reclassify him. He may have been identified as a meatcutter, but later found to be better at operating a truck. An effort is made not only to ascertain a man's proficiency in his trade if it is one that the Army needs, but also to examine, by means of psychological tests if necessary, his aptitudes for acquiring new skills in mechanical work, paper work, radio code, scout duty, or some other military specialty.

Assisting the Classification Officer at each Replacement Training Center is a so-called Personnel Consultant Officer with a background of training and experience in applied psychology, and a staff of assistants who help to carry out the program of aptitude testing, trade testing, reinterviewing, and follow-up. Conferences with the officers immediately responsible for instructing and supervising the men not infrequently suggest reclassifications, as actual try-out demonstrates that the first appraisal of a man's abilities and best usefulness should be revised. A term of eight weeks or more is ordinarily available for this period of basic military training and for such reclassification as may be indicated. Before this period is over there is entered on each qualification card the kind of duty for which the soldier's civilian experience and his military performance up to that time indicate that he is best suited, so that this information is readily at hand when personnel requisitions from tactical units and other installations are received.

During their stay in the Replacement Training Center a certain proportion of the soldiers are selected to go to schools for the training of army specialists: motorcycle mechanics, radio code operators, gunnery instructors, parachutists, bakers, airplane mechanics, or supply clerks, for example. A few with exceptional qualifications are immediately detailed to specialist duties, possibly as assistants in a dental clinic or in a personnel classification office. Also, some leaders will have emerged who may not have been identified as such by the interviewers at the Reception Centers—men whose experience before entering the Army pro-

(1) NAME (LAST, FIRST, MIDDLE) JONES, PAUL		(3) SOCIAL SECURITY NUMBER 392220106		(4) GRADE 160		(5) RACE W	
(2) BIRTHPLACE OF FATHER Chicago, Illinois		(6) MAIN OCCUPATION (CPR) Accountant (CPR)		(7) GRADE 40		(8) RACE W	
(3) BIRTHPLACE OF MOTHER Provo, Utah		(9) MAIN OCCUPATION (CPR) Accountant (CPR)		(10) GRADE 40		(11) RACE W	
(4) BIRTHPLACE OF SOLDIER Provo, Utah		(12) MAIN OCCUPATION (CPR) Accountant (CPR)		(13) GRADE 40		(14) RACE W	
(5) DATE OF BIRTH OF SOLDIER November 10, 1915		(15) MAIN OCCUPATION (CPR) Accountant (CPR)		(16) GRADE 40		(17) RACE W	
(6) CITIZEN <input checked="" type="checkbox"/> YES <input type="checkbox"/> NO		(18) MAIN OCCUPATION (CPR) Accountant (CPR)		(19) GRADE 40		(20) RACE W	
(7) EDUCATION 1937		(21) MAIN OCCUPATION (CPR) Accountant (CPR)		(22) GRADE 40		(23) RACE W	
(8) SCHOOL Provo, Utah		(24) MAIN OCCUPATION (CPR) Accountant (CPR)		(25) GRADE 40		(26) RACE W	
(9) HIGH SCHOOL Provo, Utah		(27) MAIN OCCUPATION (CPR) Accountant (CPR)		(28) GRADE 40		(29) RACE W	
(10) COLLEGE Univ. of Utah		(30) MAIN OCCUPATION (CPR) Accountant (CPR)		(31) GRADE 40		(32) RACE W	
(11) POST GRADUATE Los Angeles City University		(33) MAIN OCCUPATION (CPR) Accountant (CPR)		(34) GRADE 40		(35) RACE W	
(12) LANGUAGES, ABILITY TO CONVERSE IN English		(36) MAIN OCCUPATION (CPR) Accountant (CPR)		(37) GRADE 40		(38) RACE W	
(13) OTHER LANGUAGES None		(39) MAIN OCCUPATION (CPR) Accountant (CPR)		(40) GRADE 40		(41) RACE W	
(14) MARRIAGE STATUS Married		(42) MAIN OCCUPATION (CPR) Accountant (CPR)		(43) GRADE 40		(44) RACE W	
(15) NAME AND ADDRESS OF NEAREST RELATIVE Mrs. Josephine Jones, Mother, 5702 S. Broadway, Los Angeles, Calif.		(45) MAIN OCCUPATION (CPR) Accountant (CPR)		(46) GRADE 40		(47) RACE W	
(16) DUTY DESIGNS Accountant		(48) MAIN OCCUPATION (CPR) Accountant (CPR)		(49) GRADE 40		(50) RACE W	
(17) ARM OR SERVICE Finance Dept.		(51) MAIN OCCUPATION (CPR) Accountant (CPR)		(52) GRADE 40		(53) RACE W	
(18) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(54) MAIN OCCUPATION (CPR) Accountant (CPR)		(55) GRADE 40		(56) RACE W	
(19) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(57) MAIN OCCUPATION (CPR) Accountant (CPR)		(58) GRADE 40		(59) RACE W	
(20) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(60) MAIN OCCUPATION (CPR) Accountant (CPR)		(61) GRADE 40		(62) RACE W	
(21) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(63) MAIN OCCUPATION (CPR) Accountant (CPR)		(64) GRADE 40		(65) RACE W	
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(26) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(78) MAIN OCCUPATION (CPR) Accountant (CPR)		(79) GRADE 40		(80) RACE W	
(27) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(81) MAIN OCCUPATION (CPR) Accountant (CPR)		(82) GRADE 40		(83) RACE W	
(28) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(84) MAIN OCCUPATION (CPR) Accountant (CPR)		(85) GRADE 40		(86) RACE W	
(29) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(87) MAIN OCCUPATION (CPR) Accountant (CPR)		(88) GRADE 40		(89) RACE W	
(30) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(90) MAIN OCCUPATION (CPR) Accountant (CPR)		(91) GRADE 40		(92) RACE W	
(31) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(93) MAIN OCCUPATION (CPR) Accountant (CPR)		(94) GRADE 40		(95) RACE W	
(32) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(96) MAIN OCCUPATION (CPR) Accountant (CPR)		(97) GRADE 40		(98) RACE W	
(33) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(99) MAIN OCCUPATION (CPR) Accountant (CPR)		(100) GRADE 40		(101) RACE W	
(34) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(102) MAIN OCCUPATION (CPR) Accountant (CPR)		(103) GRADE 40		(104) RACE W	
(35) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(105) MAIN OCCUPATION (CPR) Accountant (CPR)		(106) GRADE 40		(107) RACE W	
(36) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(108) MAIN OCCUPATION (CPR) Accountant (CPR)		(109) GRADE 40		(110) RACE W	
(37) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(111) MAIN OCCUPATION (CPR) Accountant (CPR)		(112) GRADE 40		(113) RACE W	
(38) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(114) MAIN OCCUPATION (CPR) Accountant (CPR)		(115) GRADE 40		(116) RACE W	
(39) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(117) MAIN OCCUPATION (CPR) Accountant (CPR)		(118) GRADE 40		(119) RACE W	
(40) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(120) MAIN OCCUPATION (CPR) Accountant (CPR)		(121) GRADE 40		(122) RACE W	
(41) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(123) MAIN OCCUPATION (CPR) Accountant (CPR)		(124) GRADE 40		(125) RACE W	
(42) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(126) MAIN OCCUPATION (CPR) Accountant (CPR)		(127) GRADE 40		(128) RACE W	
(43) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(129) MAIN OCCUPATION (CPR) Accountant (CPR)		(130) GRADE 40		(131) RACE W	
(44) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(132) MAIN OCCUPATION (CPR) Accountant (CPR)		(133) GRADE 40		(134) RACE W	
(45) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(135) MAIN OCCUPATION (CPR) Accountant (CPR)		(136) GRADE 40		(137) RACE W	
(46) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(138) MAIN OCCUPATION (CPR) Accountant (CPR)		(139) GRADE 40		(140) RACE W	
(47) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(141) MAIN OCCUPATION (CPR) Accountant (CPR)		(142) GRADE 40		(143) RACE W	
(48) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(144) MAIN OCCUPATION (CPR) Accountant (CPR)		(145) GRADE 40		(146) RACE W	
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(56) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(168) MAIN OCCUPATION (CPR) Accountant (CPR)		(169) GRADE 40		(170) RACE W	
(57) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(171) MAIN OCCUPATION (CPR) Accountant (CPR)		(172) GRADE 40		(173) RACE W	
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(59) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(177) MAIN OCCUPATION (CPR) Accountant (CPR)		(178) GRADE 40		(179) RACE W	
(60) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(180) MAIN OCCUPATION (CPR) Accountant (CPR)		(181) GRADE 40		(182) RACE W	
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(65) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(195) MAIN OCCUPATION (CPR) Accountant (CPR)		(196) GRADE 40		(197) RACE W	
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(71) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(213) MAIN OCCUPATION (CPR) Accountant (CPR)		(214) GRADE 40		(215) RACE W	
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(73) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(219) MAIN OCCUPATION (CPR) Accountant (CPR)		(220) GRADE 40		(221) RACE W	
(74) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(222) MAIN OCCUPATION (CPR) Accountant (CPR)		(223) GRADE 40		(224) RACE W	
(75) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(225) MAIN OCCUPATION (CPR) Accountant (CPR)		(226) GRADE 40		(227) RACE W	
(76) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(228) MAIN OCCUPATION (CPR) Accountant (CPR)		(229) GRADE 40		(230) RACE W	
(77) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(231) MAIN OCCUPATION (CPR) Accountant (CPR)		(232) GRADE 40		(233) RACE W	
(78) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(234) MAIN OCCUPATION (CPR) Accountant (CPR)		(235) GRADE 40		(236) RACE W	
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(80) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(240) MAIN OCCUPATION (CPR) Accountant (CPR)		(241) GRADE 40		(242) RACE W	
(81) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(243) MAIN OCCUPATION (CPR) Accountant (CPR)		(244) GRADE 40		(245) RACE W	
(82) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(246) MAIN OCCUPATION (CPR) Accountant (CPR)		(247) GRADE 40		(248) RACE W	
(83) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(249) MAIN OCCUPATION (CPR) Accountant (CPR)		(250) GRADE 40		(251) RACE W	
(84) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(252) MAIN OCCUPATION (CPR) Accountant (CPR)		(253) GRADE 40		(254) RACE W	
(85) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(255) MAIN OCCUPATION (CPR) Accountant (CPR)		(256) GRADE 40		(257) RACE W	
(86) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(258) MAIN OCCUPATION (CPR) Accountant (CPR)		(259) GRADE 40		(260) RACE W	
(87) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(261) MAIN OCCUPATION (CPR) Accountant (CPR)		(262) GRADE 40		(263) RACE W	
(88) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(264) MAIN OCCUPATION (CPR) Accountant (CPR)		(265) GRADE 40		(266) RACE W	
(89) TALENT FOR FURNISHING PUBLIC ENTERTAINMENT None		(267) MAIN OCCUPATION (CPR) Accountant (CPR)		(268) GRADE 40		(269) RACE W	

vided little opportunity to lead but who already, within a few short weeks, have demonstrated their capacity to instruct, to take the initiative, to make prompt decisions, to organize the activities of the members of a group and to inspire their confidence. These are potential non-commissioned officers. Among them are some who may shortly be encouraged by their company commanders to apply, after they have been in the service months, for admission to an Officer Candidate School.

At the other extreme are some who prove to be very inept and slow to learn, or whose physical condition needs to be improved before they can proceed with the usual routines of military training. These are segregated in special training units where they are given the building-up exercises or the instruction best adapted to their individual needs. An attempt is made to salvage as many of them as possible for some type of army duty which they can usefully perform.

No recruit is expected to become an accomplished soldier within a few weeks. Whether as field artillery gunner or message-center clerk, rifleman or range-finder operator, he still has much to learn about weapons, field duties, and especially teamwork. But his basic training has already taught him how to take good care of himself and has hardened his physique. He has learned military courtesies, customs, and routines. He knows his rights as well as his obligations as laid down in the Articles of War. Also, the kinds of duty he can be expected successfully to undertake are now reflected with reasonable precision on the qualification card which goes with him to the tactical unit or post to which he is next assigned.

Assignment in a tactical unit

Let us follow a trainload of these soldiers as they leave a Replacement Training Center of the Corps of Engineers to fill a requisition for troops needed to bring a certain regiment to full strength. On the train we find an officer poring over some lists and tabulations—an inventory of the needs of the Headquarters Company and of each of the other companies of the regiment, and an inventory of the soldiers who have been selected at the

Replacement Training Center to fill these needs; for his is the task of balancing the regiment. That is, he must see that each company gets its share of the pace-setters and the potential leaders, and that no company is overburdened with an unduly large proportion of slow learners; for if that should happen, this one company might long delay the day on which the regiment would be fully trained and ready to move toward the zone of combat. Also, he must equitably distribute among the companies the available military specialists: the instrument men, the bulldozer operators, the truck repairmen, the draftsmen. Perhaps he finds that the Training Center has been unable to supply in full the desired complement of demolition men, and in their stead has substituted some farmers and loggers who had at least handled dynamite and done a little blasting. He notes, too, a shortage of statistical clerks, but finds some soldiers with high scores in the General Classification Test and the Test of Clerical Aptitude, and decides to ticket them for immediate detail to statistical duties.

Before the train pulls in to the siding, tentative rosters are prepared for the scrutiny of the Division Classification Officer and the officers in charge of the units among which the men will soon be distributed. And so, after only a day or two in temporary billets, the men can take their duffel bags to the barracks of the companies they are to join.

Summaries of the information on the qualification cards are promptly made available to the company commanders, to help them in getting acquainted with the men and their potentialities. The cards go to regimental headquarters where they are kept up to date and where men required for special detail can be quickly located.

So ends the narrative of initial classification and assignment. To procure the kinds of men which the different branches require and to assess and summarize their potentialities, the personnel classification system has done its best. What happens thereafter depends to a great extent on the officers of the line under whom the troops receive their further training. It remains for the commanders of the armies, army corps, divisions, regiments, com-

panies, batteries, squadrons, and platoons to see that their men are actually employed in the several capacities for which they are best qualified. To help them, the Adjutant General provides for each Division and higher echelon two assistant adjutants—a classification officer and a personnel consultant officer. Each regiment also has its personnel officer. These are the officials who, with their staffs of specially selected privates and non-commissioned officers, maintain running inventories of the available personnel, keep current the entries on the qualification cards, find men to undertake emergency duties, and help the line officers to select those who are to receive noncommissioned ratings, attend advanced schools for military specialists, or possibly go to an Officer Candidate School. The responsibilities of these classification officers will not be fully discharged until victory is won and a summary of the information about the abilities of each citizen soldier is made available to the agency which will then be helping him to secure useful and satisfying employment.

CLASSIFYING AND ASSIGNING OFFICERS

If it is important for each enlisted man to find his place of best usefulness, it is of even greater consequence that each officer should be given the responsibilities for which his training, experience, and ability as a leader show him qualified.

The Army has been filling its requirements for commissioned personnel largely from the pool of 124,000 officers available in the Reserve Corps. Regarding each of these reserve officers as well as each officer of the Regular Army and of the National Guard on active duty in the Federal Service, a thick dossier of information is on file both in the War Department and at the headquarters of the officer's Corps Area. The data summarized on his Qualification Card are obtained from this dossier, supplemented by information which he has supplied by filling out a questionnaire about recent civilian activities and experience. No attempt has been made to identify on this card the one job he is best qualified to fill, as is done in classifying the enlisted man. Instead, three broad categories of military duty are recognized: line, staff, and a specific specialty.

Under each of these headings is entered from the officer's records a description of just what he has done and how he performed the duty. His annual and general efficiency ratings and his qualifications are noted. An analysis and summary of these data, checked by representatives from the office of the chief of the arm or service to which the officer belongs, establishes his classification in each of the three categories mentioned. The information thus obtained is coded, transferred to punch cards, statistically compiled, and made available to each Army and Corps Area Commander to assist in making initial assignments, transfers, reassignments, promotions, and demotions.

After call to active duty, records are currently maintained of the officer's successive assignments and the efficiency with which these duties are performed. If he completes a refresher course for officers of his Branch, graduates from the Command and General Staff School at Leavenworth, attends the Army War College or the Army Industrial College, or has the benefit of other advanced study, such facts are entered. Available information to help in getting each officer in the right place is the Army's aim, whether he be a general officer, a staff officer, a field officer, a company officer, or a technical specialist.

OFFICER CANDIDATES

Of peculiar importance to a fast expanding army is the selection of men for training as its future officers.⁵ The supply of young graduates from the Military Academy at West Point and from Reserve Officer Training Courses in other institutions has been supplemented by establishing one or more Officer Candidate Schools for each arm and service. To attend such a school is a privilege accorded to the best qualified of the enlisted men who have already completed their basic training and had the benefit of some experience in a particular branch.

The boards appointed to sift the applications for admission to these Officer Candidate Schools have available a sum-

⁵ Edward L. Munson. *The Management of Men*, pp. xiii + 801, New York: Henry Holt and Company, 1921.

Edward L. Munson, Jr. *Leadership for American Army Leaders*, pp. xii + 96, Washington: Infantry Journal, 1941.

mary of the information on each applicant's qualification card, together with a report from his company commander and various supplementary data, including the results of a fresh medical examination. A board interviews each soldier whose application has been approved, and on the basis of all the available facts selects within the quota allowed those deemed to be most promising. For some of the schools the applicants are required to take searching examinations designed to ascertain their ability to deal with new problems, as well as their mastery of certain educational content deemed essential. But most important is such information as has been secured regarding the applicant's demonstrated ability to lead men.

The instructors in the Officer Candidate Schools are expected not only to impart the necessary information, but also to provide opportunities for training in actual leadership and carefully to observe the progress of the candidate in gaining the ability to instruct and to command. The coveted commission is awarded then to those candidates who succeed in mastering the subjects of instruction and who prove also that they are leaders.

~ OCCUPATIONAL CASUALTIES

A military force engaged in combat expects to suffer casualties. Troops may be lost, killed, wounded, captured. Such is war. Sickness and accidents, too, before and during action, deplete available man power. Is it not equally true that the effective strength of an army is depleted whenever some of its personnel is misplaced? Gross errors in classifying or assigning officers and men seriously hamper training, and in the stress of field operations, may prove disastrous.

Misplacements of personnel are the occupational casualties of war. To forestall such losses is one aim of the Army Personnel Classification System.

No system works perfectly. Instances of incorrect assignment are bound to occur during any rapid mobilization. Here, for example, is a lieutenant in the Infantry Reserve, diligent, sober, devoted to his service, who nevertheless during recent maneuvers failed pathetically as leader of his platoon. Having

demonstrated his unfitness for command, he was hailed before a Reclassification Board. By profession he is a certified public accountant. Until there is found a vacancy in the Finance Department where an officer with his qualifications is wanted, his name remains on the occupational casualty list.

Less spectacular but possibly more serious is the misplacement of another certified public accountant who is following his profession in the office which audits transactions in the procurement of munitions; for this officer has in superb degree the personal qualifications and skills of leadership and is champing at the bit for a chance to command an anti-tank battalion.

Any man is idle, whatever his occupation, if he might be accomplishing something more essential to victory. By this token, a former master plumber, experienced in supervising workmen, may be wasted working with the tools of his trade in a maintenance unit when he might be in training as leader of a machine-gun squad.

Exigencies of war result now and then in wholesale occupational misplacements. Two-faced Japan launches a surprise attack. Bombs drop on Pearl Harbor. The reverberations upset existing arrangements in many a training center. Instantly, harbor defense units and interceptor commands along our eastern and western shores must be augmented to full strength, and sentinels placed at thousands of posts previously unguarded. Soldiers being trained in various military duties are promptly rushed to these new assignments. In the emergency, what matters it that some of them had been chosen to become ski troops in a mountain regiment or selected for duty in mobile water-purification plants, or were headed toward a school for administrative assistants in base hospitals? Without regard to their civilian occupational assets, they must undertake the immediate duty, even though it may take months to heal the wounds to individual military careers.

During the first months of rapid expansion, well-laid plans for classification and assignment were hampered by the necessity of bringing certain organizations to full strength with men coming directly from Reception Centers to tactical units without

having had the benefit of military training and observation in a Replacement Center. Then, too, when interviewers, classifiers, and assignment officers new to these duties were working under tremendous pressure, regrettable errors in classification could scarcely be avoided.

Fortunately the young manhood of America is proving to be on the whole adaptable. A student of pharmacy, wanted in a medical training center, finds himself in a quartermaster training center. What is done about it? To change an assignment within any one arm or service is relatively simple; but transfer to another branch is a time-consuming, costly proceeding, and his application is disapproved. He grumbles. It is an inalienable privilege of every soldier to grouse when things are not to his liking. But eventually this young pharmacist, after instruction in the basic duties of a soldier and in the work required in a supply train, finds plenty to tax his abilities in helping to keep track of the medical supplies.

Or here is an experienced office worker who has been detailed for training as an airplane engine mechanic. At first glance this looks like an occupational casualty. It proves to be nothing of the sort, for this man has a hitherto unsuspected mechanical bent brought to light by exceptional performance in the army tests of mechanical aptitude. Like thousands of other selectees, he ultimately finds that his vocational horizon is being widened by his military experience. He gains in versatility. Resourceful, versatile workers, valued in industrial employment, are indispensable in a military force where the duties of any particular soldier or officer may suddenly have to be taken over by another member of the team.

IMPROVING PERSONNEL PROCEDURES

The Army, facing a prodigious task of personnel procurement and placement, is continuously striving toward a more practical, expeditious, and efficient system of personnel classification. Let no one imagine that the War Department is unaware of the difficulties, or that the results to date are viewed with complacency. What, then, is being done further to im-

prove the prescribed procedures and the actual operations of classification?

The story must here be told in briefest outline, for a full account would require a volume. Such a book would include a description of a freshly revised pamphlet of Army Regulations governing classification and assignment of enlisted men, an amplified code of civilian and military occupations, and a new manual for use in the training of interviewers, now in press. It would tell about current studies of occupational demand and supply, and about descriptions of army occupations and new oral trade tests recently developed with the help of another Government agency. Several chapters would be devoted to new psychological tests and objective examinations prepared in response to requests from the Signal Corps, Armored Force, Air Corps, Military Intelligence Branch, Infantry, and Quartermaster Corps for procedures to aid in selecting enlisted or commissioned personnel for specific sorts of training. One chapter would deal with procedures in selecting experienced, well-equipped soldiers for appointment as technical and administrative warrant officers—thirty different kinds of specialists—and the objective short-answer examinations which serve as one of the screens in selecting these appointees. Of general interest would be a description of standard road tests to measure the proficiency of truck drivers, and of laboratory tests to ascertain their ability to see and to drive during a blackout. Of interest also would be the follow-up studies in progress to enhance the usefulness of predictive data by providing officers with expectancy tables, critical scores, and preferred ranges as aids in choosing men for the more common types of training courses. An account of the Adjutant General's School, recently transferred to spacious quarters in Fort Washington, would have a place of prominence, for at this school hundreds of officers receive instruction in the purposes and procedures of classification.

The Adjutant General, Major General Emory S. Adams, looks to the Personnel Procedures Section of his office for aid in continuously improving the procedures and operations of the personnel classification system. Under the close scrutiny of

Brigadier General William C. Rose, Assistant to The Adjutant General, and Colonel Madison Pearson, Plans and Training Officer, it is the duty of this Section to keep in touch with operations in the field, to anticipate needs, to carry on necessary research and experimental studies, to prepare report forms, rating procedures, examinations, and other aids, and to see that they are made available as needed. This Section now has a staff of ten officers, twenty-four civilian professional workers, and fifty clerical, statistical, and administrative helpers. It also has had the help of an advisory group appointed by the National Research Council at the request of The Adjutant General in 1940 and known as the Committee on Classification of Military Personnel. Its members include Dr. C. C. Brigham of Princeton, Dr. H. E. Garrett of Columbia, Dr. L. L. Thurstone of Chicago, Dr. L. J. O'Rourke and Dr. M. W. Richardson of the United States Civil Service Commission, Dr. C. L. Shartle, Chief of the Occupational Analysis Section of the United States Public Employment Service, and the writer who serves as Chairman. They have helped in developing the various tests and in planning the army experience and training given to able selectees who eventually qualify as personnel consultant officers.

The War Department has indeed been wide awake to the vital need of conserving man power as well as matériel. Confronted with an employment problem of unprecedented proportions, it has commandeered from the Regular Army, from the Reserve Corps, and from civilian agencies some of the best brains in the country, to adapt for military purposes the most practical aids to personnel appraisal and classification.

After the first World War, personnel management, industrial psychology, vocational guidance, and employment practice made substantial advances in industry, in education, and in government, partly as a consequence of the impetus given by the Army in 1917-18. Now, a still greater conflict has given another powerful stimulus to scientific personnel studies as well as to improved practices which will make for earlier victory, and then, during years of reconstruction, may even help in building a better world.

THE RORSCHACH METHOD AND ITS SIGNIFICANCE IN THE MENTAL HYGIENE PROGRAM

MARGUERITE R. HERTZ

The Brush Foundation, Western Reserve University

Although we are prone to say that ours is the Age of Science, it is probably more accurate to say that we live in the Age of Technology. The outstanding characteristic of our intellectual climate is the mobilization of talent whereby the twentieth century has summoned its scientists from the ivory towers of pure research and put them to work forging tools and instruments.

What medicine has done in biology, and industry and business (and I may add warfare) in the physical sciences, the Mental Hygiene movement may well do in psychology.

The movement is fashioned after the Messiah of our age, Scientific Method. It derives its program from man's knowledge about man, and it seeks to apply that knowledge to problems of man in the hope of achieving and preserving the *mens sana*.

It is fitting, therefore, for the movement to say to the psychologist today:

"For your theories and your philosophies we are grateful. For your preachments and your values, we are in your debt. But what of your technology? What means have you designed for the accomplishments of your purposes? What tools have you

forged for putting your wisdom to work? Where are your tools? Where are your instruments?"

Fortunately, the psychologist is not without an impressive answer, for he has made his contribution to the century's seething output. He has brought forward techniques for study, research and diagnosis, and cautiously, he has suggested empirical methods of therapy. He can point to innumerable techniques for measuring, describing and diagnosing personality; and he can claim some success for his various psychotherapies.

It is my purpose today to concern myself with techniques for study, research and diagnosis and more specifically, with a single method for investigation into the mysteries of personality.

Innumerable techniques for measuring, describing, and diagnosing personality have been contributed. These include conventionalized paper and pencil tests, questionnaires, rating scales and laboratory techniques of measurement. In practical clinical work, most of these have been inadequate and unsatisfactory for diagnosis and guidance. Neither any single one of them or all combined can describe fully the personality of an individual or accurately diagnose the cause of even one disorder.

Small wonder, then, that in the last few decades students have sought new tools, "global" tools which view personality as a complex functioning unit, which would stress the dynamic tendencies of its constituent parts and at the same time their special uniqueness and their integration in whole life situations.

In the category of "global" instruments, fall the so-called "projective techniques" by which personality is described in terms of the individual's reactions to certain stimuli presented to him in the form of unfinished sentences, plastic materials, puppets, drawing materials, meaningful pictures, vague silhouettes, ink-blots and the like. These stimuli, when presented to an individual, have certain special meanings and values for him, depending upon his mental, emotional and experiential equipment and background.

One of the basic assumptions in projective techniques is that the individual, in reacting to the experimental stimuli presented to him, gives a sample on a small scale of his characteristic

mental and emotional behavior and social adjustment to his environment. In reacting, the individual reveals himself. He *projects* his personality. His reactions, therefore, are indicative of him, and are significant for an interpretation of his personality. By thus observing him, we gain insight into his characteristic mental procedure, his motives, interests, aspirations, abilities, emotionality, fantasy life, talents and the like. From such revelations, we are able to reconstruct his personality. The task of interpretation involves taking such products and working back to get at the basic motivations and determinants of his behavior.

The Rorschach Method (Rorschach 57, 58) is based upon this theory. Despite its comparative youth, it has challenged the interest of all whose work lies in the study of human behavior and the treatment of its pathology.

The Rorschach method employs ten standardized ink-blot, five in grey black and five with color. These are presented to the individual who is asked to tell what he sees.

The ink-blot has little pattern or structure and in responding, the individual gives forms and meanings to the blots depending upon what he sees and how he feels as he views them. Such responses constitute a projection of the subject's personality and make reconstructing it possible.

The responses to the ink-blot are studied in terms of four inquiries. First, were they in response to the whole blot or to its details? Second, what elements in the blot determined the response, those related to form, movement, color or shading? Third, what associations were inspired? Fourth, were the responses common or unusual?

These inquiries lead to Rorschach factors which describe psychological processes, and which may be summarized in a "psychogram" which in turn provides a basis upon which formal diagnosis of personality can be made. Rorschach factors are studied in terms of their frequency, their sequence, and of most significance, their reciprocal relationships. While a factor or a combination of factors may reflect general personality traits, each much be interpreted in terms of its relationship to other components of the whole.

Reconstruction of total personality by the Rorschach Method gives insight into intellectual functioning and emotionality and the interaction of the one upon the other. Thus, in addition to appraising intelligence levels, the technique tells much concerning the quality of intellectual functioning, its accuracy, flexibility, originality, and logic. Further, it reveals characteristic mental procedure in life situations and the extent of integration between imagination and rational thinking.

The emotional life of the individual is also discernible. His inner urges, drives and impulses, his fantasy life, even his anxieties and conflicts are reflected in his responses, and these may be evaluated in appraising his thinking, his creativity and his adjustment to life. In addition his emotional reactions to outside stimuli appear and may be evaluated in terms of his adjustment and control.

The method, however, does not content itself merely with isolating specific mental processes. It departs from traditional concepts by emphasizing that such processes are to be understood in terms of their interplay, each upon the other, and the degree of balance and equilibrium attained. It, therefore, differentiates "*Erlebnistypen*" or "Experience Types", as they are sometimes called, in terms of the respective roles which the inner forces (introversive trends) and outer forces (extratensive trends) play in the personality structure. The personality of the individual stands before the Rorschach investigator, illuminated by the extent to which such factors motivate or repress the individual in the quest for personal and social adjustment.

The manner in which the individual incorporates his intellectual life, his inner drives and his reactions to outside stimuli into a pattern of living which determines the level of his social competency and maturity, and the extent of his success with that synthesis are revealed by the Rorschach investigation.

This brief summary does not include all the aspects of personality which are revealed by the Rorschach Method. It serves to indicate, however, the many facets of personality which are accessible by way of the Rorschach technique (Beck, 2; Klopfer and Kelley, 36).

THE INTERPRETATION OF AN INDIVIDUAL RORSCHACH RECORD

The interpretation of a Rorschach record is a highly complicated procedure. It includes appraisal of each response as the subject gives it, an analysis of his behavior during the examination, and a quantitative and qualitative study of scores. Such study involves appraisal of scores first in terms of a norm for the group, second, in the light of their unique inter-relationships, and third, against the background of the case history of the individual.

In the first, the individual is compared with other members of his group. For example, the efficiency of his mental functioning, the strength of his emotional drives, the extent of his emotional stability, the depth of his introversion, his capacity to understand and to meet reality, all these and more are gauged in terms of the norms for his group.

In the second, the various facets of his personality are studied with reference to their relationship with each other and the effect of each upon the whole. For example, the balance between introversion and extroversion trends in the individual's personality, between his fantasy life and the sense of reality which he displays in interpreting his world, between his originality and his adaptability to the thinking of the group to which he belongs, or between intellectual control and the more spontaneous elements in his make-up, are appraised.

These two procedures are based solely upon Rorschach data and comprise what is sometimes called a "blind diagnosis". So far, we have a picture of the intellectual functioning, the social and emotional control, the maturity and adjustment of the individual and any specialized competency or disability or any unusual feature which may be present.

The third step relates the results of the first two with the personal history of the individual, deals with such factors as heredity, family background, physical health, education, and the like, and makes use of other tests and clinical data where available.

Thus, with the full case history before the Rorschach ex-

aminer, the dynamic relationship between the individual and his environment can be analyzed. The diagnosis can become more specific and more complete. The psychological forces within the personality, whether constructive or destructive, take on new meaning. His emotional reactions can be explained. The way the individual mobilizes his inner resources to meet *a* situation is expressed in terms of *the* situation. The ability with which he correlates his inner life with his overt conduct becomes apparent in terms of specific things, persons, and pressures and forces affecting him.

For example, where the Rorschach examination of a child reveals evidence of repression, revolt and anxiety, a case history of parents who are either oversolicitous or indifferent or who reject the child is significant.

Or again, indications of unusually active fantasy life coupled with a history of an excessively repressive environment may be symptomatic of basic emotional disturbance while against the background of another case history, it may indicate merely boredom in a child deprived of playmates or of a stimulating environment.

Again, absence of inner living in a small child has a meaning entirely different from that in a patient with organic brain disease.

Even discrepancies between Rorschach findings and clinical impressions have significance. For example, where emotional instability, conflicts, and anxieties are suggested by the Rorschach technique, it may very well be that clinical conclusions are based on merely superficial observations. On the other hand, even a patient suffering from an obvious mental disorder may by the Rorschach Method reveal potential capacities for adjustment.

Thus, skill in making use of interpretative material is essential. A well developed ability for configurational thinking is required of a Rorschach examiner. For, personality patterns can be understood only in terms of the various interrelationships of their component parts and in the light of the individual's personal, social, and cultural background. All this calls for an ability on the part of the interpreter to think in terms of a configura-

tion, an ability sometimes called *intuition*, without which the Rorschach record eludes interpretation.

Hence, years of experience with clinical material of all types as well as with Rorschach products are required. In this respect, however, the Rorschach Method is like any other scientific discipline. It is a tool only for him who is skilled in its use. Thus, a qualified psychologist, psychiatrist or psychiatric social worker whose professional training is adequate can become adept in its use.

It should be emphasized also that the Rorschach Method is merely one tool which may be utilized in the diagnosis and treatment of personality. Like the X-ray or electrocardiogram, it is not self-interpreting or sufficient unto itself.

In the battery of clinical methods, however, the Rorschach Method has its own values and advantages.

Its reliability and clinical validity are substantiated by impressive evidence (Hertz, 16, 18, 24; Klopfer and Kelley, 36). The interpretations of one examiner can be checked and in large measure confirmed by others similarly skilled (Benjamin and Ebaugh, 4; Krugman, 39; Hertz and Rubenstein, 29). It affords a controlled procedure which, although objective and standardized, is nevertheless sufficiently flexible to permit wide latitude of expression while obtaining information from a subject who is unaware that he is revealing himself.

Furthermore, without sacrificing the concept of personality as a single unified whole, it provides an objective picture of the interplay of the various aspects of personality each upon the other, emphasizing the dynamic balance which exists among them.

Depending upon the purpose of the examiner, the method permits of quick classification in terms of such factors as balance, emotional stability, or neurotic tendencies (Munroe, 46, 48; Harrower-Erickson, 12; Miale and Harrower-Erickson, 45). Or, it may be used to attain a fuller description including a qualitative and quantitative appraisal of the individual and his self-adjustment. When more extensive detail is desired, the Rorschach achieves its maximum usefulness as a diagnostic method

recognizing both effects and their causes and thus providing leads for treatment and guidance (Beck, 2; Klopfer and Kelley, 36).

Of course, the Rorschach Method is not infallible. Enthusiasm has led many of its proponents into pitfalls. Much has been published prematurely. Many controversial points are still subject to debate. Yet a review of the literature will reveal both careful evaluations of its contributions and cautious acceptance of its valid aspects (Hertz, 16, 18, 23, 24).

The history of the method, however, is that of any basically sound instrument which requires years of study, development, and refinement before it attains complete scientific status. We can venture that evidence steadily accumulates that it has met the pragmatic test. It works. If properly used by trained workers, it is one of the more important methods of personality diagnosis available today. It reveals more about the functioning personality in a shorter time than any other single instrument at our disposal.

Its possibilities in the Mental Hygiene program are challenging. While an exhaustive discussion would require more time than is now available, certain applications of the method may be suggested.

EFFICIENT TOOL IN THE MENTAL HYGIENE PROGRAM

In the field of child guidance, Mental Hygiene stresses the necessity of understanding the mental and emotional growth of the child, of knowing his fundamental needs, of appreciating the essential unity of these needs in the integrated personality. It emphasizes the adjustment of the activity and the tasks of the individual to the personality and stage of development of the individual. It calls for a more adequate diagnosis of problem children not only in the light of the immediate problem but in terms of basic personality difficulties. It recommends treatment by proper control, balance, use and guidance of the specific mental, emotional, and social capacities of the individual child.

In all these phases of child guidance, the Rorschach makes its unique contributions. Studies employing the Rorschach Method as a research instrument have furnished valuable data

on the personality development of children of preschool and school age, of adolescents and of college students. Thus, applying the method to children in various stages of development, Rorschach patterns in an individual record may be compared to norms for the specific age groups which are available and any unusual feature may be related to developmental findings.

Rorschach studies on the mental approach of children to their world, for example, reveal characteristic mental reactions at various age levels. Klopfer and Margulies (37), for example, enumerate three characteristics of young children up to three years of age; "magic repetition" or a reaction given to the first blot which serves as a sort of "magic key" for succeeding blots, rejection, and perseveration. After three years, responses are more varied. Klopfer (35) has also demonstrated that certain aspects of mental approach such as "contamination" and "confabulation", indicative of distorted thinking in the adult and reminiscent of adult schizophrenic reactions, may appear during certain phases of the normal child's mental development.

Brush studies of children 6 and 8 years of age reveal characteristic differences in the perceptual approach to the world. Children, 6 years of age, are prone to view whole situations in a broad general manner without analysis, concentration on the specific parts or on the relationship between these parts. They also grasp the large, common features in an objective situation. Some children are prone to single out unimportant details in an objective situation, ignore their relationship to the rest of the situation and hence misinterpret it.

Eight-year-old children likewise embrace the whole of an objective situation but show ability to analyze it into its essential features. They tend to react differentially to the different aspects of the situation and hence show the beginnings of mature patterns of mental procedure. From 8 years on, there is greater accuracy, organization, complexity, and originality in the perceptual approach (Hertz and Ebert, 28).

Other studies confirm these findings and reveal further differences (Hertzman and Margulies, 32; Kay and Vorhaus, 34). Thus many reactions in young children which to the adult mind

appear distorted and irrational must not be construed as disorders in perceptual or conceptual thinking. On the contrary, they are normal reactions in the early years.

Rorschach studies also show that emotional instability, excitability and impulsiveness are natural up to eight years of age. Thus when the Rorschach record of a child of six years exhibits patterns of emotional stability, it may not mean happy adjustment as his parents are apt to report. The child may be precocious or overadjusted. As a matter of fact, invariably other patterns in the Rorschach record show a high degree of repression, a kind of passive submission to authority. As Klopfer would say, the child is "a willing victim of an overdose of discipline" (Klopfer and Kelley, 36, p. 251).

Again, sequential studies at the Brush Foundation have established certain emotional patterns of adolescence. Reference must be made to previous articles which describe these studies (Hertz, 20, 21, 22; Hertz and Baker, 26, 27). Suffice to say that considerable data have been amassed which have proven highly practical in application.

It is shown, for example, that there is a characteristic increase in inner living and fantasy life and a decrease in objective attitudes and emotional responsiveness toward the world from 12 to 15 years of age. Twelve-year-old children are characteristically more extratensive, more "outward" in their emotional behavior, more responsive to the environment, and less dominated by inner living. They are more objective and impersonal in their reactions to the environment. By the time they are 15 years old, they are much more introverted, more concerned with their inner world and their subjective experiences, more constrained and less outwardly responsive. By 15 years, they also show greater mental control and emotional adaptability and a cautious guarded approach to their environment which is at times depressively toned.

Curiously, there is much evidence of egocentricity, emotional instability and impulsiveness in girls at 15 years of age, although these patterns do not outbalance the more adaptable emotions. But it must be accepted that these are normal mani-

festations of the period when the sex impulses take on additional urgency and significance.

Further studies of girls at 12 and 15 years of age reveal significant differences depending upon pubescent status. Comparing 12 and 15 year old girls of like pubescent status, for example, it is shown that older pubescents are reliably more introverse and expansive in personality but less stable emotionally than the younger. Attainment of late puberty appears to be accompanied by an introversial swing and heightened emotions. Thus, personality changes discernible within the age range of 12 and 15 years of age appear to be due not to pubescence *per se*, or age *per se*, but to the interaction of physical change, age, social experience, and social pressure which is operative upon the maturing girls (Hertz, 17).

I might cite a case in which such information was highly valuable. A fifteen-year-old girl showed alternating spells of anxiety and depression on the one hand and egocentricity, independence, and hostility on the other. At times, she was most affectionate, obedient and companionable; at other times, rebellious, defiant and difficult. She wanted "to do big things" and because she had not achieved them at the age of fifteen, she felt "that nothing was worthwhile". Because of morbid brooding and depressed spells, failure in school and refusal to participate in social activities, the psychiatrist was consulted.

The condition was diagnosed as reactive depression.

The Rorschach record indicated superior intelligence in a highly ambitious individual. She had the ability to achieve, and a strong drive for intellectual conquest, but she was not capable of using her productive capacities for actual achievement. She was impractical; she failed to display common sense. She frequently lacked emotional control. Many infantile patterns showed in the record, peculiarly in conjunction with mature thinking and independent habits of mind.

Fortunately, she showed potentialities for good mental control and social adjustment.

Comparison of many of the patterns in the record with norms for her age group, suggested in various French studies and in

research at the Brush Foundation, revealed that many of the so-called neurotic traits were not unusual for her age. Tension, violent mood swings, oversensitivity, cautiousness, anxiety, depressed feelings, withdrawal from the world, escape into fantasy, clinging to childish behavior and exercising independence at the same time, are all characteristic of this age. And, they are frequently accompanied by poor scholastic performance.

From the case history we learned that the girl had an over-dominant mother, a more submissive father, and a highly intellectualized environment. Other unstable family relationships complicated the picture and intensified the girl's maladjustment. Her goals were definitely too high, made that way by a neurotic parent who was too demanding, who treated her as an adult, consulted her about her professional work, and generally made the girl feel that she had to be an adult to be worthy of her confidence. Such demands on this developing child caused unusual emotional stress.

It was consoling to be able to report to the psychiatrist that the personality traits which appeared to deviate from the normal, were not unusual, infrequent or unhealthy at this age. Even the vascillating behavior which appeared to give the family so much trouble was indicative of ambivalent attitudes frequent at this age. They had impelled the girl to demand adult freedom while clinging childishly to her parents for protection. The inner private world of this adolescent assumed a potency and a significance which was a normal expression of the growth period.

This case is cited to indicate how the Rorschach permits consideration of various aspects of personality in relation to the developmental phase in which they occur. Treatment involved changing the environment and the people in it rather than attempting any radical changes in the girl herself. With improvement in the neurosis of the mother, the girl improved. Within a year, serious conflicts and anxieties disappeared, ambition became more proportionate to ability, and development appeared to progress in a more wholesome way.

Thus it may be seen that the Rorschach Method furnishes valuable data on the normally developing personality, and con-

tributes in no small measure to our understanding of the child's development.

The Rorschach also proves itself valuable in psychological clinics and guidance centers where personality disorders and behavior problems are presented. Where there is a personality maladjustment, fundamental causes are often imbedded in the innermost self of the individual, and the Rorschach, because it can probe such depths, is a valuable adjunct to any clinical procedure (Krugman, J., 39; Krugman, M., 40, 41).

The Rorschach frequently elicits personality data not readily accessible to the clinician. Psychologists today, for example, lay emphasis on the role of intelligence in the adjustment of the individual to himself and to his world. Mental ratings are almost always included in any psychological or psychiatric study. But significant also is the kind of intelligence the individual possesses, his special abilities and disabilities, the extent to which he lives up to his native potentialities and the influence of his emotional nature on his thinking.

The Rorschach is unique in revealing this type of information. It not only gauges intellectual level, it also detects special leanings and deficiencies. It tells much about important qualitative aspects of intelligence,—the flexibility of thinking, susceptibility to the minute, predominance of abstract or concrete abilities, uncritical appraisal of situations and any predominance of abstract or concrete abilities. The method shows the capacity of the individual for planning and for achievement and the quality and the evenness of his intellectual performance. It also reveals work habits, imaginativeness, zeal, productivity, and originality. These are appraised in terms of the entire personality "Gestalt".

Where an individual is intellectually one-sided, highly analytical and meticulous, for example, the Rorschach reveals whether it is a natural trait or a neurotic symptom. It tells whether the individual is intellectually curious, a keen observer, a worker in details, or whether he has developed compulsive traits and must dwell on every small and insignificant part of a situation. It tells whether he is flighty and intellectually undisciplined or neu-

rotically evasive and fleeing from reality, or suffering psychotic disintegration.

Where there is active fantasy life, the record shows whether it is integrated into a process of thinking and meeting reality and used for creative accomplishment or whether it forms the basis for the morbid introspections of the anxious depressed individual or for the autistic productions of the distorted mind.

The individual may show deficiency in organizational ability. The record will also reveal whether this is due to native defect as in the subnormal, deterioration as in brain diseases, or inhibition as in the emotionally overwhelmed individual.

Again, an individual may show accuracy and precision in thinking, fine powers of discrimination, capacity for concentration, and mental control and discipline. The Rorschach will indicate how wholesome these characteristics are in that individual. They may represent superior intellectual functioning, or they may indicate an excessive effort for control by the subject straining convulsively to give a good performance, frequently seen in the pedantic or in the depressed type of individual.

The Rorschach proves of great help also where discrepancies occur between two psychometric tests or when school performance is at variance with test results, for the method differentiates between potential capacity and actual efficiency. An intelligence test score may point to borderline or low intelligence but the Rorschach will distinguish among native deficiency, arrested development and malfunctioning.

In cases of intellectual retardation, we have often had the experience of differing from other test reports or from psychiatric diagnoses. In one case, anti-social behavior was ascribed to low mentality. The Rorschach showed low efficiency level rather than low intelligence. It indicated, among other things, neurotic repression with depressive features. It was evident from the rest of the picture that the child was not functioning to capacity.

Another child was assigned a low intelligence quotient on the basis of the Stanford-Binet Test. The Rorschach record showed basically good mental abilities functioning along constructive and imaginative lines but intellectual functioning was

inconsistent and illogical at times due to anxieties and emotional blocking. Spontaneous and creative impulses were suppressed and the child clearly was not working at maximum efficiency.

At times, there is a discrepancy between the achievement scores and intellectual level, the achievement being higher than intellectual capacity warrants. This is often explained in terms of Rorschach findings of strong intellectual drive and tremendous ambition, perhaps, or of great perseverance and good work habits.

But intellectual considerations alone provide an incomplete picture. A vivid imagination, a strong drive, an intense emotional impulse, a sense of security or anxiety, all color the individual's control and adjustment. The Rorschach analysis captures these more elusive aspects of personality. It tells whether the inner forces of the individual are sufficiently well integrated to function adequately. It describes an individual's drives and ambitions, telling us whether they are put to constructive use or are merely compensatory for feelings of insecurity. The conceited, striving individual, for example, stands apart from the wholesomely ambitious person, while lack of ambition can be recognized as based upon either native defect or psychogenic inhibition.

The method likewise reveals valuable data on the strength and adequacy of the individual's outer emotional reactions. We may note how the individual uses his emotions in making his adjustments, whether he is able to establish wholesome relationships to people about him and to what extent he depends upon the outer world for his needs and satisfactions.

We may likewise study the balance between these inner and outer forces, the extent to which impulses and inner drives are integrated with the demands of the environment. How is this achieved? Does he externalize his emotional energy so that he satisfies environmental demands and at the same time his personal needs?

Where imbalance is indicated, we gauge what form it takes. Perhaps the realities of the inner world are too compelling and the individual lives in a "private world of feelings and ideas"

and is rooted within himself, withdrawn and seclusive. Perhaps he succumbs to his fantasies or to morbid brooding.

The individual may, on the other hand, be unable to control his more primitive emotions and consequently gives free rein to his drives and impulses. He may react violently to people and to things. He may be too dependent upon the environment for his pleasures and satisfactions.

Intellectual control may be inadequate to balance his feelings and emotions. He may not be able to accept the reality of the outside world, especially in his personal relationships. Or, he may be overcontrolled, inhibited and inflexible in his emotional reactions. He may repress the spontaneity of his emotional reactions and adopt a coldly impersonal manner of dealing with situations. He may be blocked by anxiety, or constricted by serious compulsive tendencies.

With such analysis, new areas of tensions, emotional disturbances, unsuspected fears, conflicts, sex disturbances, insecurities, anxieties and inadequacies are uncovered. All these help immeasurably in understanding maladjustment.

The Rorschach is likewise helpful in attacking problems of social adjustment since traits of personality are revealed which play significant roles in the relationships between the individual and his environment. We learn, for example, of the extent to which the individual is responsive to his environment, of his ability to adjust to reality and to group life, and of his capacity for sympathetic understanding of his fellows.

Even in its capacity to probe the inner life of the individual, the Rorschach throws light upon social attitudes. We may learn, for example, of the individual's capacity to control his emotions and to channelize them into socially acceptable forms of expression, and of the manner in which he integrates forces in the environment with those within himself. We may learn whether he is assertive, aggressive, dominating, argumentative or belligerent, or whether he is complaint, resigned or submissive to external pressures. Thus we are able to probe for negativistic, stubborn, rebellious attitudes or undue recklessness, lack of re-

straint, timidity, excessive cautiousness and suggestibility. And we may detect destructive and antisocial attitudes.

It can be seen readily how such analysis gives us insight into difficulties in social adjustment. We have had many cases of social maladjustment where Rorschach results helped considerably by showing that emotional immaturity, instability, frustration, compulsive tendencies, hidden but intense sex interests, fears, distrust, or anxieties were dominant features in the personality structure.

The Rorschach is especially helpful in equivocal cases. A few instances perhaps may illustrate how Rorschach results contributed to a better understanding of the personalities involved. Of course, in mentioning cases, only the more important facts can be included in these brief summaries.

B was a child of ten, of superior intelligence, with high academic grades and a school history of good adjustment, who suddenly became shut in, inaccessible and introverted. Teachers reported a quiet child, likeable, highly conscientious about her work, apparently well adjusted in the school room.

The Rorschach revealed a child of superior intellect, highly analytical and critical, accurate and precise in her thinking. She was highly cooperative, a "conformist". Significantly, the Rorschach also showed rich imagination and fantasy life, creative ability and originality which failed to appear in any other test data or in the case history. Indeed, her teachers reported that they never would have associated such traits with her. The Rorschach also revealed serious repression and inhibition, lack of self confidence, insecurity and anxiety. Her personality was predominantly introversive but fair ability for extratensive adaptation was also indicated.

It was evident from the information in the case history plus Rorschach findings that school work and activities had not given the patient satisfaction or status and hence had affected her self confidence and social adjustment. She was capable of more productive creative accomplishment. Instead, all natural spontaneity and creative activity had been so repressed that she had lost her initiative. She had lived up to school standards of achievement

and conformity and hence her social adjustment was considered good. But for her, it was a passive adjustment, a complaint attitude which was personally unsatisfying and hence not healthy.

When the child's condition was suspected, teachers and parents tried to encourage the child to "get away from herself", to make outside contacts, to join clubs and to participate in out-of-door activities. The Rorschach picture suggested a different mode of attack. Rather than attempt to develop extrovert activities, recommendation was made to attune work and play to the abilities and interests of the child which were decidedly introverse, since this would give her more adequate satisfaction.

In another case, we had a boy aged five with periodic spells of severe stuttering. He had high intelligence, was apparently happy and adjusted in the home, had many playmates and participated in many group activities. Stuttering had begun at the age of three years after a serious illness.

Case history revealed a highly intellectualized home.

The Rorschach record indicated superior creative intelligence, rich imagination, and an abundance of emotional energy. The record showed practical judgment and superior ability to handle concrete problems with precision and ingenuity. It also revealed great striving, an inordinate drive toward complicated performance which was pushed beyond his ability. It disclosed a seriously disturbed child, basically tense, anxious, and insecure, characteristics theretofore unsuspected.

Supplementary tests revealed high mechanical aptitude and many mechanical interests.

Among other things, the combined test material indicated serious disturbance, anxiety, in a child who was continually striving to live on a high intellectual plane, to be in tune with the parents, although interests, aptitudes, and talents were in another direction. Valuable leads were provided for further diagnostic study and treatment. The therapy recommended consisted of furnishing a home atmosphere which would give the child security, reduce emotional stress, supply channels for emotional release and for expression more in harmony with his basic needs.

In another case, the Rorschach contributed to the under-

standing of the problem of a college student who requested help because he was unhappy. He had a brilliant mind, had made splendid grades, had high intellectual standards, and was considered one of the more "successful" students on the campus. He felt that he was not getting along with others and that he needed guidance. His advisors considered the case not serious because "a person of his superior attainments cannot expect to make good adjustment to people of more mediocre ability around him".

Then he broke. He became morbid, moody and depressed, withdrew from social contacts, then from classes, and finally left school.

The case history revealed that he had few close friends. They reported that he was admired but he was "too highbrow". "He always had to use big words and just had to discuss intellectual things." "At the slightest provocation, he engaged in philosophical and religious speculation." He also had the annoying habit of asking his friends the definition of unusual words.

The Rorschach record revealed superior mentality, high mental endowment, differentiated intellect, keen observation, sound judgment, rich imagination, all in an emotionally repressed and inhibited individual. He was tense, anxious, insecure, and incapable of making adequate social adjustment.

It was apparent from the record that the boy was unable to deal realistically with social situations. He shied away from them. He denied his emotions free play. He suppressed spontaneity. He selected that medium through which he could attain more comfortable intellectual activity and through verbal and intellectual gymnastics, he found release for his feelings. He, therefore, confined his efforts to intellectual activity, taking refuge from social difficulties, much to the annoyance of his fellows. He was unhappy, not so much because he was not making adequate social adjustment as he thought, but because he was not emotionally mature nor freely expressive.

Nowhere in the other test results, in the case history, or in the initial psychiatric report was there any indication of this lack of feeling tone. Once it was suggested by the Rorschach,

psychotherapy was started in the effort to bring about more adequate emotional expression and more wholesome social contacts.

These brief cases illustrate the value of the method in delving into the inner life of the individuals and in providing clues to the unique organization of personality. Because of its sensitivity, the method is used not only in the clinic, but also in school and college adjustment (Munroe, 46, 49), vocational guidance and counselling (Piotrowski, 54, 55; Piotrowski and Candee, 56; Zulliger, 59, 60, 61). With the aid of the information furnished, the individual can be guided to realize to the fullest his potential capacities and to use them to face reality. He can be helped to choose goals within his potentialities. He can be directed so that his emotional energy will attain full and adequate expression.

The method is employed now in planning programs of study and recreation. It is incorporated in college guidance programs to estimate the intellectual and emotional needs and resources of students.

Its value for vocational guidance has been considered only recently. Reference has already been made to the power of the test to diagnose specific abilities and talents. In its capacity to identify the person with philosophical bent, the abstract thinker, the creative writer or artist, the person with a scientific type of mind, and the overmeticulous plodder, the method can be of aid in vocational guidance. In its ability to provide cues as to special interests, play activities, and hobbies, it is helpful in indicating vocational preferences and possibly vocational aptitudes.

Recently, the method has been employed for the evaluation of aptitude in flight training (Bigelow, 5) and for the selection of subjects able to withstand physical discomfort (Hertzman and Seitz, 33) and mental discomfort (Piotrowski, 55). Certain occupational patterns have been identified for the selection of outstanding mechanical workers (Piotrowski and Candee, 56). Results of these studies warrant further exploration along these lines.

At present, however, the greatest contribution by the Rorschach method in this area is its capacity to supply information

on the kind of functioning personality an individual possesses. Problems in industry, business and labor relations are largely problems of personality. Whatever special fitness an individual has for the task at hand, such traits as insecurity, instability, inferiority, anxiety and other forms of maladjustment, hinder adjustment and vocational efficiency.

The Rorschach reveals such personality traits and hence suggests those which help or hinder vocational adjustment. It frequently uncovers abilities and traits of special significance in adjustment and vocational success, and at times, unrecognized capacities. Piotrowski (54) has recently suggested Rorschach patterns for the detection of psychological traits essential to educational and vocational success of adolescents. These Rorschach patterns tell something about the kind of drive the individual possesses, his capacity for planning and for working toward specific goals, his basic attitudes toward the world, the quality and evenness of his performance level, persistence of his effort, his relationship to his own group and to authority in general, and the degree of responsibility which he can assume.

It can be seen readily how the Rorschach record itself without any specific recommendations concerning vocational aptitudes or talents would be helpful to the counsellor. Such elements as lack of mental control, inferior logic, poor persistence, inability to sense what is important and what is secondary, unusual stereotypy in thinking, extreme and unbalanced introversion, poor rapport with the world, inability to get along with people, repression, excitability, inability to do routine work, neurotic tendencies and the like, indicate at once that the individual is a poor risk for certain professions and occupations. Despite the fact that research in the application of the Rorschach to vocational guidance is in its infancy, it promises to make important contributions in this field.

It is in the psychopathological area that the Rorschach method has proven of greatest assistance to the clinician (Beck, 3; Klopfer and Kelley, 36). Rorschach patterns which are characteristic of particular types of mental disorders have been identified. Indeed, some of these patterns are so characteristic of cer-

tain diseased conditions, that they are accepted today by some authorities as pathognomic signs. Specific signs in certain combinations, for example, reflect the schizophrenic process; others, psychoneuroses; still others, intra-cranial brain damage. Today, the method is a valuable adjunct in clinical differential diagnosis of mental deficiency, schizophrenia, psychoneuroses, depressive states, convulsive states and intra-organic pathology.

The method is invaluable in its diagnostic capacity, if it only confirms psychiatric reports. Of course, it is especially helpful in equivocal cases or when psychiatrists cannot agree.

In our experience it has proven of especial value in detecting fears and anxieties, phobias, sex disturbances, fanaticism, and even fetichism which failed to appear in other psychological and psychiatric data. We have had cases of children outwardly appearing well adjusted to their environment, displaying serious tension, fears, and anxieties in the Rorschach records. In one case of inattention and failure in school, the Rorschach alone gave interesting leads as to anxiety and sex disturbances due to suppression of sex interest, masturbation, and frantic attempts at deception to circumvent discovery. In another case of a highly cultured woman who suffered periodic depressive spells which baffled psychiatrists, the Rorschach revealed as no other test instrument, associations so impregnated with eroticism that all her thoughts and emotions were colored by it.

We have had several suggestions of "peeping" in adolescents and adults from the Rorschach record alone, valuable findings because the examiner could recommend investigation along these lines. In one rather spectacular case, the Rorschach record served to detect a delinquent because it revealed fetichism in an 18 year old boy for whom "shoes" incited such overwhelming sexual desires that he broke into homes and stole them. Not only were "women's shoes" seen in various parts of the blots, but in unusual areas, and were stimulated by the sensuous qualities of the blots. In addition, other responses especially those before and after the revealing content, indicated neurotic shock, anxiety and serious conflicts.

Many clinicians find the Rorschach especially helpful in

differentiating between neurotic and prepsychotic states, neurotic and psychotic conditions, schizoid traits and schizophrenic conditions, depressive states and introversion, true and neurotic fears (Krugman, 40). Many studies have been recorded where the method helped psychiatrists distinguish between anxiety neuroses, or organic brain disorder, or mental deficiency and schizophrenic symptoms.

The Rorschach is especially sensitive in detecting incipient deviations when they are not noticeable clinically. In our experience, we have detected cases of early schizophrenia before clinical diagnosis was made. We have had several cases of mal-adjusted children who have come to the attention of court or clinic for delinquencies where the Rorschach suggested the onset of a neurosis or psychosis before it was clinically observed. In the diagnosis of intra-cranial organic lesions, the method is often the first to reveal personality disorganization and at once suggests the need for more specific diagnostic procedures.

The greatest contribution in this area, however, is in the light which the method throws on the psychological structure which underlies the various kinds of clinical behavior, the insight it gives into the mechanisms which are used, its power to appraise the degree and trend of deviation and the extent of integration and deterioration. More than any other single instrument, it reveals, for example, the intellectual and emotional deterioration in the schizophrenic process,—impaired judgment, uncritical interpretation of events, regression of personal interests, autistic thinking and dissociation, delusional ideas, bodily pre-occupations, the diminution of appropriate emotional contact with reality, emotional variability and instability or emotional blunting, seclusiveness, anxieties, negativism, and blocking.

It should be emphasized that in all these respects, the Rorschach is more than a diagnostic instrument. It is also an aid in determining the kind of guidance best fitted to the individual and the choice of therapeutic treatment. For example, where the record reveals extreme impulsiveness with otherwise good indications of healthy development, rather than to suppress or destroy this energy, the record suggests directing it into socially

sanctioned patterns of conduct. Where, on the other hand, the record reveals lack of control, irresponsibility, impaired judgment and other infantile emotional patterns, the necessity of rigid supervision is at once suggested.

Again, when we know from the Rorschach record as in the case of *B* (*supra*, pp. 668-669), that an individual is basically introverted and has good capacity for inner emotional adjustment, guidance and treatment must involve use of these inner resources. Social rapport will be more difficult if the patient is forced to respond to his environment directly.

In one case of a child showing extreme egocentricity and exhibitionism, the Rorschach also revealed good intelligence with marked imaginative, creative and artistic ability. Among other recommendations, it was suggested that the child be given reasonable opportunities for self expression, probably by giving her a responsible position in a club or by encouraging participation in dramatics. It was suggested that this would be a constructive way of meeting the child's needs and at the same time giving her proper opportunity for recognition.

In another case of extreme antagonism, negativism and rebellion to authority, the Rorschach showed high intelligence, clear logical thinking, marked powers of criticism, and tremendous emotional energy and drive. It was suggested that the drive be utilized and directed in a socially useful way, that the individual be encouraged to participate in group discussions or to join a debating club.

In another case of a girl of fifteen in whom serious disorientation was suspected because of over-indulgence in fantasy life, the Rorschach revealed along with imaginative and creative productivity, good mental control and adequate social adjustment. Despite the fact that the girl seemed to dissociate herself from reality, she was really well oriented.

Her history showed that her home environment was not sufficiently stimulating to her. She had substituted a life of fantasy because her inner life was more vivid and attractive than her outer life. Further, her originality, which bordered on eccentricity, appeared to shock her parents to her great satisfaction.

Indeed, she seemed to get such pleasure out of their discomfort that she revelled in her eccentricities.

Recommendation was made that the girl be guided and helped to reconcile her inner demands and creative strivings with reality. It was suggested that she be permitted to direct children's groups, to tell them imaginative stories, to write these stories and send them to children's magazines, and to join a theatre group. Thus her imagination would become an integral part of her activity; she would profit from the joy of achievement, and at the same time, she would gain social recognition and acceptance.

The Rorschach method serves another purpose. It may be given before, during, and after guidance and treatment and thus it may be used to gauge the effectiveness of the therapy. Many clinicians employ the method as a check-up.

We have encountered cases in which patients were seriously disoriented and disturbed upon the first test. Later tests showed marked changes so that records with distinctly abnormal features came to manifest better adjustment. In the second record, inner life appeared well integrated with mental activities dealing with outer reality, while capacity for external emotional adjustment and for emotional control was discernible. In some cases, introspective ability and a high degree of sensitivity to the environment showed. Abnormalities tended to disappear while judgment and intelligence improved, all pointing to the return of emotional adjustment. While clinically at the time of the second test, the patients may have failed to show improvement, after a few weeks, improvement became noticeable.

On the other hand, in some cases, check-ups frequently reveal an increase in psychological difficulties. Further mental impairment may appear; poorer control, less adjusted emotions, dominance of more primitive urges; anxiety and confusion may make their appearance.

Sometimes subsequent tests show improvement in one sphere only, perhaps the intellectual. This would indicate that while the patient is capable of a wider range of intellectual activity, he is still emotionally inhibited from functioning adequately.

It is in its prognostic power, perhaps, that the Rorschach has its greatest possibilities. Knowledge of the dynamic relationship between intellectual capacities, emotional functioning and social adaptability, permits prediction of impending personality changes important in educational and vocational planning and clinical prognosis. In this respect, the Rorschach Method contributes in no small measure to preventive mental hygiene.

Experimentation and clinical studies have identified Rorschach patterns which tend to be constant in personality structure and those which are subject to change (Piotrowski, 53). Of course, the individual's situation in life and the pressures and challenges of the environment must be known before any predictions are attempted. With this knowledge, however, the possibility of change in the direction of adjustment or maladjustment may be anticipated from the Rorschach record.

Thus, Piotrowski (53) shows that where the so-called constant factors are intact, the prognosis is good for adequate adjustment. Evidences in a Rorschach record of insight, self-criticism, sound intellectual functioning, effort, planning, rapport with the world, emotional acceptance of reality, mental and emotional control, and social adaptability, permit prediction as to personality integration and social adjustment. On the other hand, where the more constant patterns are deficient, the possibility of adjustment is more remote.

Zulliger (59) has assembled a battery of Rorschach patterns which signify greater or less educability for school children, which has proved of great practical value. Batteries of personality traits have also been identified which permit prediction of substantial accuracy of the success or failure of college students by Munroe (46, 49), of medical students by Harrower-Erickson (13) and of nurses, by Piotrowski (55).

It is probably in psychopathology that the method shows greatest promise as a prognostic aid. Probably its greatest usefulness when applied in the field of mental hygiene is the aid it gives in early diagnosis or in the discovery of incipient personality defects. In addition, many Rorschach patterns suggest whether the patient is capable of education, of developing in-

sight, and of making a satisfactory adjustment to reality.

In cases of brain concussion, for example, Rorschach results enable one to gauge the extent of deterioration (Oberholzer, 50). With schizophrenics, it has been demonstrated that where the more constant traits of personality are defective, improvement is less likely. The easier, the deeper, and the more adequate the emotional reactions, the more favorable the prognosis (Piotrowski, 51, 52). Thus Piotrowski could predict the effectiveness of insulin from pretreatment records and Harrower-Erickson (11) could prognosticate from postoperative Rorschach records of organic patients whether improvement would take place even when the output was restricted and when discouraging signs of abnormality were still in evidence. Again, a closer relationship to every day reality, fewer antagonistic tendencies, greater emotional control and less anxiety are considered good prognostic signs in chronic alcoholism by Billig and Sullivan (6).

It is in this area, that is, in predicting personality development and subsequent behavior, that the method is most promising.

In connection with another problem of mental health, delinquency, the Rorschach has been invaluable. It has been used in the analyses of children showing early behavior difficulties and in the clinical study and treatment of offenders.

It is readily seen how it can be powerful in efforts to prevent delinquency. As already indicated, it has power to diagnose early mental deficiency and to detect such traits as poor mental control, lack of restraint, and inadequate judgment, and to reveal maladjusted personalities with their mental conflicts, emotional disturbances, compensations for feelings of inferiority, repression of sex impulses and the like. It can also detect early symptoms of mental disorders which, under certain circumstances, are sure to lead to criminal acts.

I might cite innumerable Rorschach analyses which disclosed mental conflicts, compensatory activities, sex disturbances, obsessions, compulsive features as prominent factors in the records of offenders. And, as already indicated, on several occasions we have seen the Rorschach indicate the onset of psychosis which

was expressed in delinquent acts, not suspected by court or clinic.

The Rorschach method is also put to diagnostic use in determining the educational adjustment, the recreational programs, and vocational guidance of inmates of correctional institutions. It is employed for classification and segregation of the mentally deficient and disordered in many institutions and prisons (Lindner and Chapman, 43; Lindner, Chapman and Rinck, 44).

Thus, the prognostic possibilities inherent in the method make it serviceable in determining whether the individual should be returned to society and whether he is capable of making the necessary social adjustment.

I can mention, only in passing, another field in which the Rorschach is making rapid progress, in the classification, selection and screening of military personnel and in the diagnosis, prognosis and treatment of abnormal states induced by modern warfare.

It can readily be seen that success in aviation, for example, depends not only on technical skills, but also on such psychological factors as intelligence, originality, persistence, confidence, tenacity, emotional stability, and adaptability. These factors may be studied through the Rorschach record. It is, therefore, employed today in the selection of aviation personnel (Bigelow, 5). Again, certain types of personality disclosed by the Rorschach, such as the anxious, the suspicious, the hypochondriacal, or the highly introversive may be identified as not fit for certain service units.

The military uses of the Rorschach have been amply discussed by Harrower-Erickson (9, 13). Many war studies of its use in diagnosing mental disorders have already been published (Abbott, Due and Nosek, 1; Brussel, Grassi and Melniker, 7; Brussel and Hitch, 8).

Because of the need to examine large masses of individuals, the Rorschach Method has been modified into a group technique, and although it is still in its experimental stage, the group Rorschach has already proved of great service (Harrower-Erickson, 10, 13, 14; Hertz, 19, 25; Hertzman, 30, 31; Lindner, 42; and Lindner and Chapman, 43).

I have sought thus briefly and in a general way to outline the potentialities of the Rorschach Method as it can be brought to the assistance of the Mental Hygiene program. Certainly social workers are quick to appreciate an approach to personality which insists upon it as a totality embracing component parts functioning in adjusted harmony. Traditionally, your emphasis has been upon problems of adjustment as revealed by symptoms rather than upon the symptoms themselves.

Your concern has been with the individual's total adaptation to his environment; your problem has been to improve that adaptation. You are, therefore, interested in identifying not only deficiencies but also their causes and you seek to treat those causes. You also recognize differences in personal capacities and much of your work is based upon the hope that the individual can be brought to better self-adjustment.

The Rorschach Method may, therefore, well aspire to become your instrumentality. It can give you new insight into the motivation of human behavior. It can assist you in diagnosis and by that diagnosis guide you in treatment and reconditioning. Such of your everyday problems as marital discord, delinquency, vocational maladjustment, emotional inadequacy, sexual abnormalities, and psychotic behavior, take on new meaning when they are related by the Rorschach Method to maladjustments within personality. In a recent study, Krafft and Vorhaus (38) have demonstrated how valuable the method proved itself when employed in this way by a family case-work agency.

Moreover, its usefulness is not limited to diagnosis and suggested therapy. It can be valuable in forecasting what you may dare to hope for; or as Krugman (40) has phrased it, it can appraise "the treatability" of the client. And it can also help you in recognizing when and how well your job has been done.

Consequently, the method has gained widespread acceptance among many psychologists, psychiatrists and social workers. Hospitals and clinics, penal and correctional institutions, social settlements and child guidance centers, schools and personnel departments and social agencies of many kinds have employed it and found it valuable. In short, all who have occasion to seek

insight into the dynamics of behavior, social workers, psychologists and psychiatrists all working together can employ it with other tools, to learn to understand that global entity, the human personality.

With the hopefulness which alone saves us in these dark days, we can look forward to its ever-growing usefulness. We dream of a peace to come, and of a world order in which the nations of men may be reconciled in a system of mutual respect and adaptation. Probably we dare to hope because we dare not do otherwise.

It may be that the hopefulness with which we view the Mental Hygiene movement is born of a similar need to hope, and that the promise which some of us find in our humble ink-blots is akin to it.

But to one principle at least, we must hold fast, that the salvation of man depends upon man's ability to master himself. The disposition to regiment men into masses and to treat their problems as mass problems only, is a threat to our survival. An approach which re-emphasizes the dignity of the individual and the sanctity of his unique personality, offers the soundest basis for our hopes and aspirations, for men can live at peace with others only when they are at peace within themselves. The peace of which we dream, the world order for which we pray, depend upon achieving harmony not only among nations, but within them, among men and women.

Once again we must arm ourselves to fight for the faith that the hopes of mankind can be realized only through adjustment of the individual. In that struggle, the Rorschach Method may well become a most effective weapon.

BIBLIOGRAPHY

1. ABBOTT, W. D., DUE, F. O., and NOSIK, W. A. Subdural hematoma and effusion as a result of blast injuries. *Journal of the American Medical Association*, 121:739-741. 1943.
2. BECK, S. J. Introduction to the Rorschach method: A manual of personality study. *American Orthopsychiatric Association Monograph*, No. 1, pp. xv + 278. 1937.
3. BECK, S. J. The Rorschach test in psychopathology. *Journal of Consulting Psychology*, 7:101-111. 1943.

4. BENJAMIN, J. D., and EBAUGH, F. G. The diagnostic validity of the Rorschach test. *American Journal of Psychiatry*, 94:1163-1178. 1938.
5. BIGELOW, R. B. The evaluation of aptitude for flight training. The Rorschach Method as a possible aid. *Journal of Aviation Medicine*, 11:202-209. 1940.
6. BILLIG, O., and SULLIVAN, D. J. Prognostic data in chronic alcoholism. *Rorschach Research Exchange*, 6:117-125. 1942.
7. BRUSSEL, J. A., GRASSI, J. R., and MELNIKER, A. A. The Rorschach Method and postconcussion syndrome. *Psychiatric Quarterly*, 16:707-743. 1942.
8. BRUSSEL, J. A., and HITCH, K. S. The Rorschach Method and its uses in military psychiatry. *Psychiatric Quarterly*, 16:3-29. 1942.
9. HARROWER-ERICKSON, M. R. The contribution of the Rorschach Method to wartime psychological problems. *Journal of Mental Science*, 86:366-377. 1940.
10. HARROWER-ERICKSON, M. R. Modification of the Rorschach Method for use as a group test. *Rorschach Research Exchange*, 5:130-144. 1941.
11. HARROWER-ERICKSON, M. R. Personality changes accompanying organic brain lesions: III. A study of preadolescent children. *Journal of Genetic Psychology*, 58:391-405. 1941.
12. HARROWER-ERICKSON, M. R. Diagnosis of psychogenic factors in disease by means of the Rorschach Method. *Psychiatric Quarterly*, 17:57-66. 1943.
13. HARROWER-ERICKSON, M. R. Large scale experimentation with the Rorschach Method. *Journal of Consulting Psychology*, 7:120-126. 1943.
14. HARROWER-ERICKSON, M. R. The use of the Multiple Choice Rorschach in clinical medicine. Paper presented by the American Psychopathological Association, May 15, 1943. New York City.
15. HARROWER-ERICKSON, M. R. and MIALE, F. R. Personality changes accompanying organic brain lesions; pre- and post-operative study of two preadolescent children. *Rorschach Research Exchange*, 4:8-25. 1940.
16. HERTZ, M. R. Evaluation of the Rorschach Method and its application to normal childhood and adolescence. *Character and Personality*, 10:151-162. 1941.
17. HERTZ, M. R. Personality changes in 35 girls in various stages of pubescent development based on the Rorschach Method. *Psychological Bulletin*, 38:705. 1941.
18. HERTZ, M. R. Validity of the Rorschach Method. *American Journal of Orthopsychiatry*, 11:512-520. 1941.
19. HERTZ, M. R. Comments on the standardization of the Rorschach group method. *Rorschach Research Exchange*, 6:153-159. 1942.
20. HERTZ, M. R. Personality patterns in adolescence as portrayed by the Rorschach ink-blot method: I. The movement factors. *Journal of Genetic Psychology*, 27:119-188. 1942.
21. HERTZ, M. R. Personality patterns in adolescence as portrayed by the Rorschach ink-blot method: III. The "Erlebnistypus" (A normative study). *Journal of General Psychology*, 28: 225-276. 1943.
22. HERTZ, M. R. Personality patterns in adolescence as portrayed by the Rorschach ink-blot method: IV. The "Erlebnistypus" (A typological study). *Journal of General Psychology*, 29:3-45. 1943.
23. HERTZ, M. R. Rorschach: Twenty years after. *Psychological Bulletin*, 39:529-572. 1942.
24. HERTZ, M. R. The Rorschach Method: Science or mystery? *Journal of Consulting Psychology*, 7:67-79. 1943.
25. HERTZ, M. R. Modification of the Rorschach ink-blot test for large scale application. *The American Journal of Orthopsychiatry*, 13:191-211. 1943.

26. HERTZ, M. R. and BAKER, E. Personality changes in adolescence as revealed by the Rorschach Method. "Control" patterns. Paper read before the Mid-western Psychological Association, Ohio University, Athens, Ohio. April, 1941.
27. HERTZ, M. R. and BAKER, E. Personality patterns in adolescence as portrayed by the Rorschach ink-blot methods. II. The color factors. *Journal of General Psychology*, 28:3-61. 1943.
28. HERTZ, M. R. and EBERT, E. The mental procedure of 6 and 8 year old children as revealed by the Rorschach ink-blot method *Rorschach Research Exchange*, 8:10-30. 1944.
29. HERTZ, M. R. and RUBENSTEIN, R. A comparison of three "blind" Rorschach analyses. *American Journal of Orthopsychiatry*, 9:295-315. 1939
30. HERTZMAN, M. Recent research on the group Rorschach test. *Rorschach Research Exchange*, 7:1-6. 1943.
31. HERTZMAN, M. Comparison of the individual and group Rorschach tests *Rorschach Research Exchange*, 6:89-108. 1942.
32. HERTZMAN, M. and MARGULIES, H. Developmental changes as reflected in Rorschach test responses. *Journal of Genetic Psychology*, 62. 1943.
33. HERTZMAN, M. and SEITZ, C. P. Rorschach reactions at high altitudes *Journal of Psychology*, 14:245-257. 1942.
34. KAY, L. W. and VORHAUS, P. G. Rorschach reactions in early childhood. Part II. Intellectual aspects of personality development. *Rorschach Research Exchange*, 3:71-77. 1943.
35. KLOPPER, B. Pseudopsychotic reactions in Rorschach records of preschool children *Psychological Bulletin*, 38:597. 1941.
36. KLOPPER, B. and KELLEY, D. M. *The Rorschach technique*. Yonkers-on-Hudson: World Book, pp. x + 436. 1942.
37. KLOPPER, B. and MARGULIES, H. Rorschach reactions in early childhood. *Rorschach Research Exchange*, 5:1-23. 1941. ++
38. KRAFT, M. R. and VORHAUS, P. G. The application of the Rorschach method in a family case work agency. *Rorschach Research Exchange*, 7:28-35. 1943.
39. KRUGMAN, J. I. A clinical validation of the Rorschach with problem children. *Rorschach Research Exchange*, 6:61-70. 1942.
40. KRUGMAN, M. Rorschach examination in a child guidance clinic. *American Journal of Orthopsychiatry*, 11:503-512. 1941.
41. KRUGMAN, M. The Rorschach in child guidance. *Journal of Consulting Psychology*, 7:80-88. 1943.
42. LINDNER, R. M. A further contribution to the group Rorschach. *Rorschach Research Exchange*, 7:7-15. 1943.
43. LINDNER, R. M. and CHAPMAN, K. W. An eclectic group method. *Rorschach Research Exchange*, 6:138-146. 1942.
44. LINDNER, R. M., CHAPMAN, K. W. and RINCK, E. C. The development of a Group Rorschach Technique in a Federal Penal Institution, with special reference to the problem of psychopathic personality. *Psychological Bulletin*, 39:513-514. 1942.
45. MIALE, F. R. and HARROWER-ERICKSON, M. R. Personality structure in the psychoneuroses. *Rorschach Research Exchange*, 4:71-74. 1940.
46. MUNROE, R. The use of the Rorschach in college guidance. *Rorschach Research Exchange*, 4:107-130. 1940.
47. MUNROE, R. Inspection technique: a modification of the Rorschach method of personality diagnosis for large scale application. *Rorschach Research Exchange*, 5:166-190. 1941.

48. MUNROE, R. An experiment in large-scale testing by a modification of the Rorschach method. *Journal of Psychology*, 13:229-263 1942.
49. MUNROE, R. Use of the Rorschach in college counseling *Journal of Consulting Psychology*, 7:89-96. 1943
50. OBERHOLZER, E. Zur Differenzialdiagnose organisch-psychischer und psychogen bedingter Störungen nach Schädel-und Hirnstraumen vermittels des Rorschachschen Formdentversuchs. Proceedings, First International Neurological Congress. Bern, 1931.
51. PIOTROWSKI, Z. A. A simple experimental device for the prediction of outcome of insulin treatment in schizophrenia *Psychiatric Quarterly*, 14:267-273. 1940.
52. PIOTROWSKI, Z. A. The Rorschach method as a prognostic aid in the insulin shock treatment in schizophrenics. *Psychiatric Quarterly*, 15 807-822. 1941.
53. PIOTROWSKI, Z. A. The modifiability of personality as revealed by the Rorschach method: Methodological considerations. *Rorschach Research Exchange*, 6:160-167. 1942.
54. PIOTROWSKI, Z. A. Tentative Rorschach formulae for educational and vocational guidance in adolescence. *Rorschach Research Exchange*, 7:16-27. 1943.
55. PIOTROWSKI, Z. A. Use of the Rorschach in vocational selection *Journal of Consulting Psychology*, 7:97-102. 1943.
56. PIOTROWSKI, Z. A. and CANDEE, B. Rorschach signs in the selection of outstanding mechanical workers. Paper read before the Eastern Psychological Association, April, 1942. To be published.
57. RORSCHACH, H. Psychodiagnostik. Methodik und Ergebnisse eines wahrnehmungs-diagnostischen Experiments (Deutenlassen von Zufallsformen.) (Psycho-diagnostics. Methods and results of an experiment in perceptual diagnosis by means of interpretation of random forms.) (3rd ed., ed. by W. Morgenthaler.) Bern: Huber, 1937. Pp. 225.
58. RORSCHACH, H. Psychodiagnostics; a diagnostic test based on perception. (Trans. by P. Lemkau and B. Kronenberg.) Bern· New York; Hans Huber; Grune and Stratton, Pp. 226. 1942.
59. ZULLIGER, H. Der Rorschachtest im Dienste der Erziehungs—und Berufsberatung. *Gesundheit und Wohlfahrt*, 14:273-286 and 309-327. 1934.
60. ZULLIGER, H. Jugendliche Diebe im Rorschach—Formdeutversuch. Berne (Haupt), pp. 166. 1938.
61. ZULLIGER, H. Einführung in den Behn-Rorschach-Test. (Introduction to the Behn-Rorschach Test). Bern: Hans Huber, 1941.

THE PATIENT AND HIS PERSONALITY

A short discussion of the Rorschach method of personality evaluation and its uses in clinical medicine.

M. R. HARROWER-ERICKSON *

*Research Associate, Department of Neuropsychiatry,
University of Wisconsin*

The Rorschach method of personality evaluation requires on the part of the "subject" or person tested that he give his impressions of, or responses to, ten standard inkblots—black, white and coloured—which are printed on cards 9 by 7 inches. These ten inkblots are shown to him one by one with the question "What does this look like to you, or what might it be?" The individual's task therefore is simply to tell the examiner what these inkblots remind him of, or look like.

The role of the examiner is to take down verbatim what the subject says, question him as to certain characteristics of his answers; score, evaluate and tabulate these answers, and make his interpretation of the individual's psychological equipment on the basis of the information which he gets.

The very natural reaction of someone reading the above for the first time is likely to be sceptical and disparaging. It is just too simple to make sense: how can one learn anything of importance about a person's character and personality from his responses to a set of inkblots? Moreover, who is Rorschach, and why should one use his inkblots?

Before discussing the test therefore, it may be well to answer some of these initial questions. Hermann Rorschach, a Swiss psychiatrist, experimented for some ten years, comparing the

* Formerly of the Montreal Neurological Institute.

responses given by normal persons in various walks of life, with those of psychotics, neurotics and various abnormal personalities, to the same inkblots. Just before his early death in 1922 he published a short account of his findings which he called a method of "Psycho-diagnostik"; very naturally the test subsequently acquired his name.

That the procedure is simple no one will deny; but before we discuss what underlies this apparent simplicity it is well to remember that many clinical procedures of tried and tested value are in themselves very simple, and moreover may look quite meaningless and trivial to persons without the necessary background. Divorce yourself for a moment from your acquired medical background and training, and, with the eyes of the layman watch a neurologist tickling the sole of the patient's foot with a match (even to describe this to yourself as "eliciting the plantar reflex", or Babinski, means that you are carrying over part of your training). Does it not seem just too simple to be possible that a diagnosis of disease of the pyramidal tract can be made because the big toe turns up instead of down? Many other examples could be given to illustrate the same point. If we really look in an unbiased manner at any experimental or clinical technique, we find that without the necessary background, knowledge and experience, even the most important and established procedure may appear trivial. Conversely, once one has the necessary background, a very simple procedure may be profoundly significant and allow of far-reaching conclusions.

Granted, you may say: but still, why inkblots? They are meaningless. That is, of course, the essence of the whole idea. Rorschach deliberately created something without meaning in order that any sense, or organization which was given to it, would come from the individual who was looking at it. If the same haphazard, accidental splotch looks to one person like a rabbit, to another like a map, to a third like a cloud, to a fourth like a flower, to a fifth like a man and so on, then we know—quite regardless of what such responses may mean—that what went on between the moment when the *same physical stimulation*, the same light rays, hit all their retinæ, and the moment

when they communicate to us their *experience*, must have been different in all these cases. It is these differences that we are interested in, for they indicate different mental processes.

Perhaps the easiest way to make this point clear is to contrast the type of information which can be derived about a person from the Rorschach method with that which is derived from the majority of psychological tests. Most of the tests which you may have encountered, as for example, the Binet intelligence test (from which the I.Q. is derived) or the non-language performance tests, are composed of questions or tasks for which there is one right answer. The individual either gets the right answer, and receives credit, or he does not get it, and fails in that item. It does not matter how he got the right answer, nor does it matter why in other cases he fails to get it. The score is concerned only with the correct performance; and the estimate of his ability will be in terms of his score.

Now in contradistinction to this, in the Rorschach there are no right or wrong answers *per se*. Moreover, each answer reveals something about the processes which produced it.

Let me give you an analogy from another field. Suppose that you have three test tubes each containing an unknown and colourless liquid, and you want to find out as much as possible about these fluids from one test. The usual type of psychological test can be likened to such a performance as testing each of your tubes with a bit of blue litmus paper. In such a test there are only two possible alternatives; either the paper changes colour or it does not. In either case you have an answer to the question of acidity. But you might also investigate the liquids in a way which allowed them to exhibit more of their essential properties; let us say you add some solution which brings about an entirely different reaction in each case. In one producing a change in colour, in the second causing it to give off a gas, in the third making it cloudy. These different reactions would depend on the properties of the liquids, and the fact that they inter-reacted with your test solution in these different ways would give you information as to their composition.

Now in the inkblots we really have the equivalent of such

a test solution which allows of an infinite number of different reactions, reactions which in their turn depend on the particular qualities of the mind we are studying.

We have therefore a new kind of mental measuring rod, one that throws the burden of proof upon the individual, forcing him to display his mental wares of our inspection.

Now, in very general terms, what kind of information about a person do we obtain? One way of describing it is this: we can estimate the "range richness, or variety of an individual's psychic reactivity, the degree of his social or affective adjustment, the extent to which his inner life (that is his own resources, regardless of stimulation from the environment) are integrated and constructive, or disorganized by more primitive drives and undue anxiety. We also learn the strength of his intellectual control, the rigidity or plasticity of his mental approach, his way of tackling problems. We get in short an insight into the dynamics of the given individual".³

The term "dynamics" is more than just a metaphor, for in looking at an analyzed Rorschach record one might be looking at the dials or indicators in some big power house. That is, we are looking at the relative strengths of various drives, needs, impulses, instincts—call them what you will—and at how strong the various controls or disciplines are which keep these energies in check. The sum total of these controlling mechanisms and energies may result in equilibrium, a physical system working smoothly—a well adjusted individual; or they may result in disequilibrium, an explosion—the maladjusted individual; or in too little power—the depleted, impoverished personality.

So much for some of the general ideas underlying the Rorschach method. The question of the technique of scoring and of interpreting the scored results, however, is much more difficult to discuss briefly.

Scoring, or analyzing a record is done for the purpose of breaking up the ingredients, or showing what perpetual processes have been at work in any given response. Each response, and let us say there were five to each card, 50 in all, must be considered from four points of view. The first is, *where* on the inkblot did

the spotlight of your attention focus? Did you look at the whole of the blot and tell me that it looked like a bat, for instance, or did you look at certain parts of the blot, finding in them, be they big or small, the things you were reminded of?

Secondly, what made you see what you did? If in one of the coloured cards you had seen a sweet pea, was it the colouring of the sweet peas in your garden? Was it the shape of the sweet pea petals? Was there some delicate shading which reminded you of the texture of a flower petal? Within this category of 'what determines your response' there are thirteen possibilities, shape, colour, texture, a feeling of movement being some of them.

Then there is the question of the content of your answers, what *things* did you see? Were you concerned with anatomical drawings, with animals; with flowers, the scientific instruments, with people, or the thousand and one other common objects which make up the world we see around us. Quite obviously there can be a great variety in this category.

Finally there is the question of how often you see what other people see, in the particular part of the blot in which they

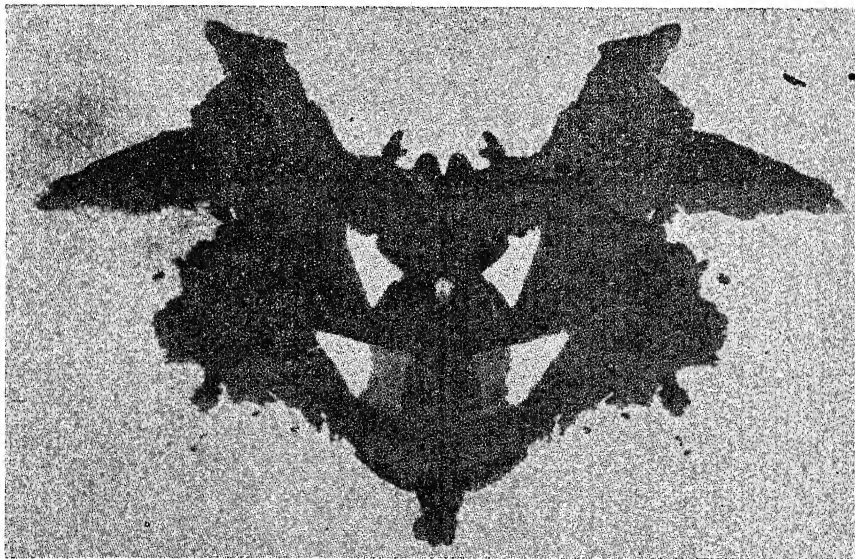


FIG. 1

see it. Or, otherwise expressed how usual, or unusual is your answer.

One and the same answer, then, or shall we say what appears at first to be the same answer, may after proper analysis prove to be the result of very different mental organization, and, as such would be scored differently and have a very different meaning.

Suppose two people gave the response "Bat", to blot I, which is reproduced here. Superficially one is inclined to assume that they have both had identical experiences but that may be far from true. One person may have seen the whole blot as a bat, with wings outstretched as if in flight. He will readily admit that it is approximately, if not quite, the shape of bats he has seen, and that he feels the creature is drawn "as if it were in motion." But the second individual may have completely disregarded the major part of the blot; his attention may have been riveted on the two small clawlike knobs at the top centre; to him these may have had the feeling of bats' claws about to attach themselves to something. Moreover the dark colouring may have given him the feeling of darkness, and thereby, bats, but he will refuse to accept the blot as the shape of a bat. These two seemingly similar responses, therefore, will be shown to differ as to the "where" of the response, as to the "determination" of the response, and in the frequency with which such a response is given. They have in common only the content, which it so happens, is frequently of much less importance than the others. Now a valid interpretation of the results will depend on accurate scoring of responses, and this can only be done if one is able to discover *how* the individual sees what he tells you. Eliciting this information takes place in the "enquiry", a free discussion of the subject's answers when he has completed his spontaneous responses.

Despite the impossibility of discussing the scoring of a record in detail in this short space, there is one aspect, the psychogram, which allows itself to be considered somewhat superficially, and can be used to demonstrate differences in scored records even to persons without Rorschach training. The psychogram is merely a way of tabulating certain aspects of the responses

so that the relative strengths of various factors, their relationships, the presence or absence of certain psychological ingredients, can be seen at a glance. Since there is no one normal type of record, just as there is no one normal type of individual, the illustrative example in figure 2 is of necessity somewhat arbitrary. It is the record of a "normal" person.

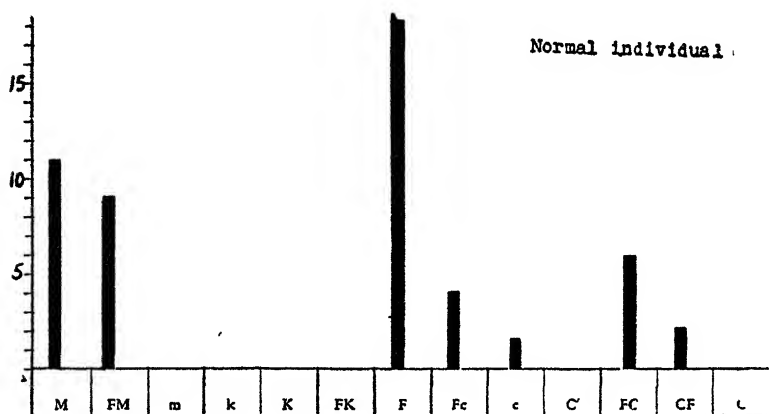


FIG. 2.

Each response of a given kind is scored as a unit and marked above the various notations. This individual having 11 M responses, 9 FM, etc.

In the accompanying table a rather dangerous (*because oversimplified*), summary of the significance of the various notations has been attempted. It will, however, allow of some landmarks so that the various psychograms can be compared.

Granted, therefore, that we have a psychological test of a different kind, and different degree of sensitivity from the usual, what use can be made of this in clinical medicine? Quite obviously the average patient is not primarily a "problem child", nor is information concerning his mental make-up of outstanding importance.

But there are undoubtedly patients, for the understanding of whose condition a Rorschach examination may be of value, and these can be divided into two general classes.

TABLE 1

<i>Notation</i>	<i>Response involving</i>	<i>Absence of response</i>	<i>Correct proportions</i>	<i>Errors</i>
C	Colour only			Violent emotions, uncontrolled
C'	Black-white only			Residue from emotional shock. The "burnt child"
c	Texture only			Preoccupation with bodily sensations, consciousness
OF	Colour and outline	May mean lack of emotional depth	Powerful emotions, emotional depth	Explosive emotions, temper
F	Outline, or form only	Lack of control or self-discipline	Stability, control	Compulsive, tense
Fc	Form and texture	May mean lack of tact and consideration	Fine-grained, tactful, sensitive	Ultra-sensitive, self-conscious
FC	Form and colour	Lack of enriching contact with the environment	Successful, rich life with others	The glad-hander
FK	Form and perspective		Awareness of one's own motives, insight	Undue introspection
FM	Form and movement (Animals in action)	Lack of, or repression of instinctive drives	"Drive", aliveness dynamic, energetic	Swamped by instinctive drives, immature
k	Surface depth (X-ray)			Anxiety on specific issues
K	Diffusion (clouds, smoke)			Vague, general anxiety
M	Form and movement (Humans in action)	Lack of enriching inner resources	Rich imaginative inner life; self-sufficiency	Retreat into inner life, lack of contact with reality
m	Form and movement (Inanimate objects)			Repression, difficulties in relation to inner life

LEGEND TO TABLE 1.

As was mentioned in the text, a short summary of this kind is apt to be misleading because it is over-simplified. It is impossible for example to state how many responses of a given kind must be considered 'excessive' or in the correct amount, for the role played by any one type of response is dependent on its relation to other responses. A record which showed four CF responses, which did not have FC and M responses, and showed few F's, would be considered as showing an individual with obviously explosive and uncontrolled emotional energy, but this *same amount of CF* is occurring in a record with, let us say, 8 M responses, and 6 FC responses would indicate an entirely different psychological equipment. (The scoring symbols, or the notations, used in this article are those which have been developed by Bruno Klopfer in the *Korrbach Research Exchange*, and more recently in his book entitled *The Korrbach Technique*.)

There are cases on the one hand where the clinical diagnosis has still to be made; where a differential diagnosis must be arrived at; and on the other there are cases where, even with the clinical situation clearly understood, added information about the patient's psychological potentialities is desired in order to clarify the situation. In the first type of case the chief task of the Rorschach examiner is to recognize in a given patient's record those aspects which we have come to associate with a particular clinical condition. For instance, as I have discussed at length elsewhere² the records of persons with cerebral tumours have been shown when analyzed, to conform to a consistent pattern and to be remarkably similar one to another. Such similarity does not mean that all persons with a cerebral lesion will see the *same things* in the inkblots, far from it, but rather that analysis of their responses reveals certain mental processes to have been at work *to the exclusion of others*. This similarity is arresting because, as a general rule, the Rorschach records of normal persons, even when analyzed, differ greatly one from the other.

In the same way we have arrived at certain characteristics which one might say belong to the neurotic or unstable individual³ and have utilized 9 "neurotic signs" in an evaluation of a record. When more than four "neurotic signs" are found in a given record the patient is under suspicion, and the question arises whether his physical condition may not be directly connected with his impoverished or maladjusted personality.

For this first group of patients, therefore, a Rorschach examination will provide information which can be considered added evidence for or against a provisional clinical diagnosis. If one patient is a brain tumour suspect, or if for another psychogenic factors are suggested as all important, a Rorschach record which shows both patients to have normal well-adjusted, well-developed personalities will speak against the presence of a cerebral lesion in the one case, and against a purely psychogenic basis of the physical symptoms in the other.

Conversely, if one patient shows the typical, restricted personality which is the counterpart of cerebral damage, and the other shows one of the various neurotic maladjustments, then the

Rorschach diagnosis would have confirmed or added additional evidence to the original diagnosis.

For the other class of patient there will be no help needed in the diagnosis. It is obvious, let us say, that this patient has epilepsy, but how can we help him to adjust to the particularly difficult situation which is facing him; what aspects of his personality can we trade on, in helping him to help himself? ^{4, 5}

Or again, in the case of this obviously paranoid individual, what ideas or constellation of ideas will force themselves into the meaningless medium of the inkblots. Can we get a clue to some aspects of the disturbance which may have not come to light in the psychiatric interview?

Yet, again, admitting that this patient is neurotic, how fundamental is the disturbance, what chance has psychotherapy to rehabilitate him, what line of approach should be adopted?

We may now turn to some actual psychograms; those in figure 3a and 3b are the types most commonly found in patients with cerebral lesions, regardless, strange to say, of the location of the lesion. If either of these two psychograms is compared with that of a normal individual, as for instance with that in figure 2, it will be noticed that they differ from it greatly. Figure 3a represents responses which, in addition to being considerably reduced in quantity, are limited to one mode of perception only. Moreover the white units represent a type of reaction not found in the normal record, namely, vague, unjustified or unsubstantiated forms, which are frequently the result of an almost automatic preservation of one idea regardless of the actual properties of the inkblot.

Figure 3b shows that while some important modes of responding other than the F type are still possible, that they are reduced to a minimum.

Restated in more general terms both these psychograms indicate that the patient's range of psychic reactivity, both in regards to his environment and dealings with others, and in terms of his own inner life, have been reduced to a minimum.

The three patients whose records are epitomised in figure 4a, b, c present a common type of impoverished personality, which is

FIG. 3a .

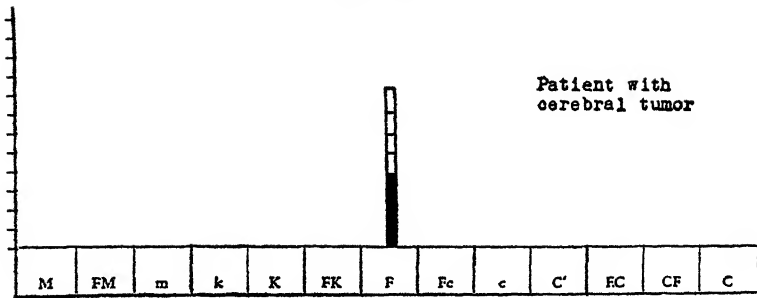
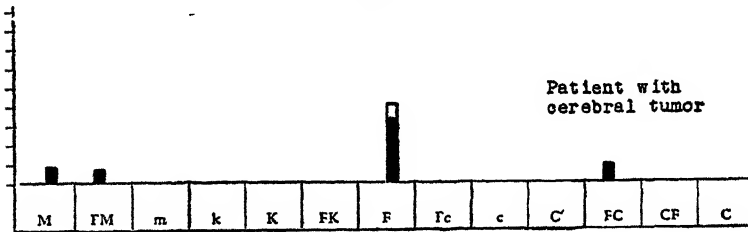


FIG. 3b



correlated clinically with a diagnosis of psychoneurosis, generally anxiety neurosis or neurasthenia. Describing such a personality we would say that it was impoverished because, while showing no deterioration, it is without inner resources and without adequate contact with the environment.

FIG. 4a

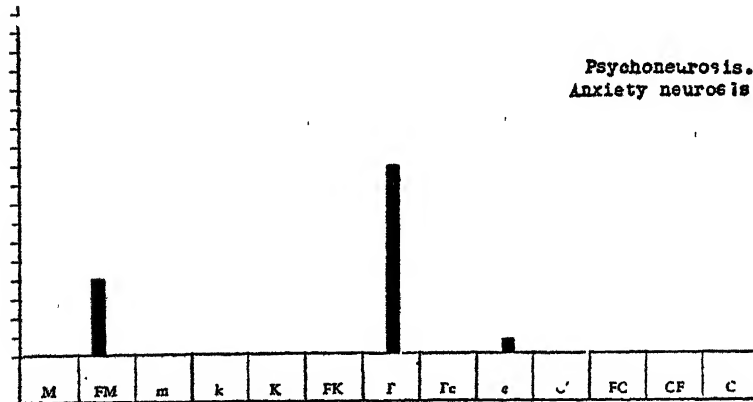


FIG 4b -

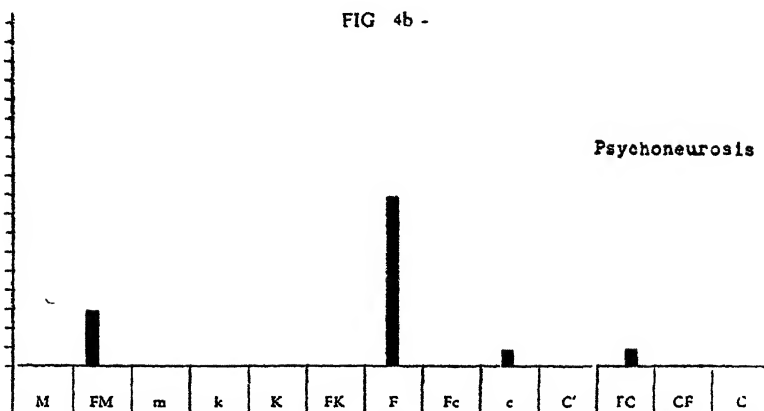
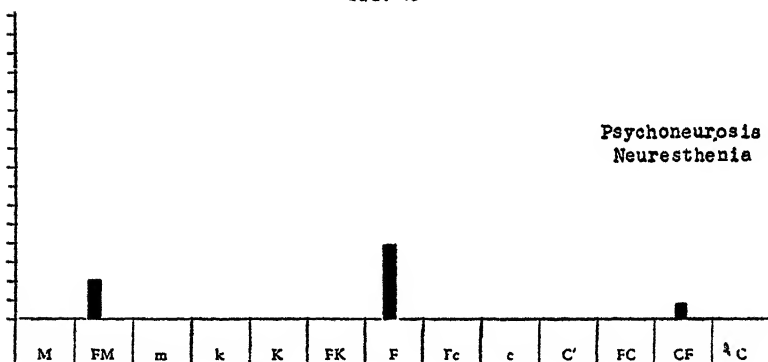


FIG. 4c



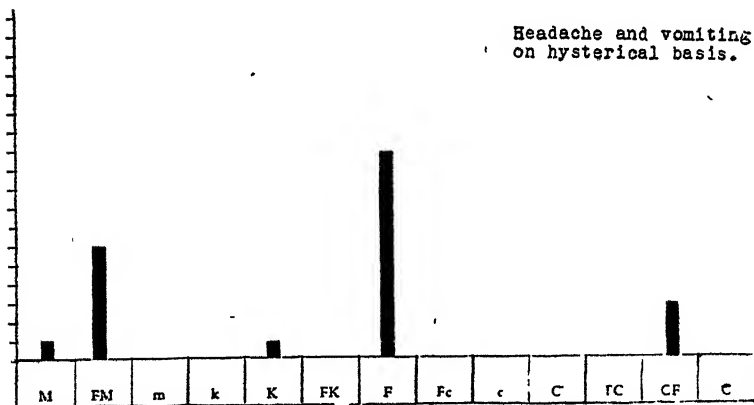
Despite the fact that these three patients complained of very different physical symptoms a clinical diagnosis of psychoneurosis was given in each case. From the Rorschach standpoint, the psychological ingredients are practically the same in all cases. When these records were further analyzed in terms of the neurotic signs already referred to, it was found that in two cases all nine of the signs were present, and eight of the nine signs were present in the record of the third.

Such cases are amongst the least ambiguous with which we have to deal. But it is not always necessary that as many as eight or nine signs be present before the personality appears as inade-

trear Neurological Institute with some 70 patients concerning whom the question of a neurosis has arisen, shows that among persons with five or more signs, 71% has proven to be neurotic, 24% neurotic with some somatic disability in addition, and only 5% have been finally considered clinically as having some exclusive somatic disturbance. In other words, where an inadequate personality is discovered in the Rorschach this inadequacy bears *some* relation to the general physical condition in almost all cases.*

Let us now consider figures 5a, b, and c. Here are three patients with very different symptoms—headache and vomiting (5a), paralysis (5b), and epileptic attacks (5c) who all prove finally to be conversion hysterics. These three cases are instructive in contrast to the previously discussed cases because it cannot be argued that the psychograms are similar. At first sight figure 5c may appear to you to be normal, while figure 5b may look like the restricted record of a case of cerebral tumour; 5a probably falls on superficial inspection into the category just discussed, the impoverished personality. Why has our Rorschach diagnosis been

FIG. 5a



* W. Donald Ross⁶ has shown that in a group of 42 neurotic patients studied by him, 74% showed 5 or more of our Neurotic signs. He has also called attention to the high incidence of these signs in a group of persons of sub-standard rating, that is, of low intelligence and insecure economic status.

FIG 5b

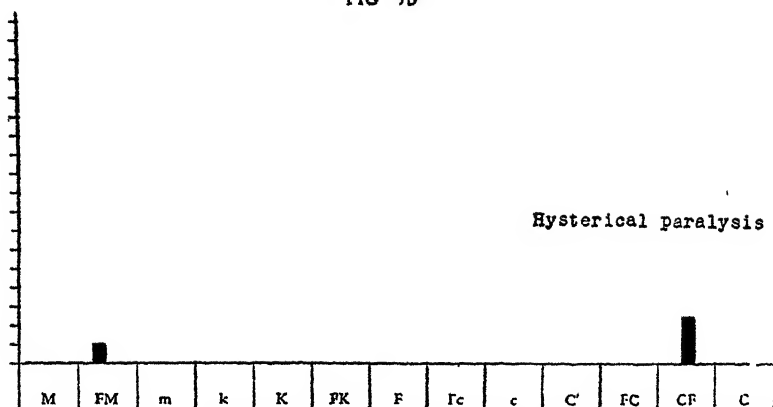
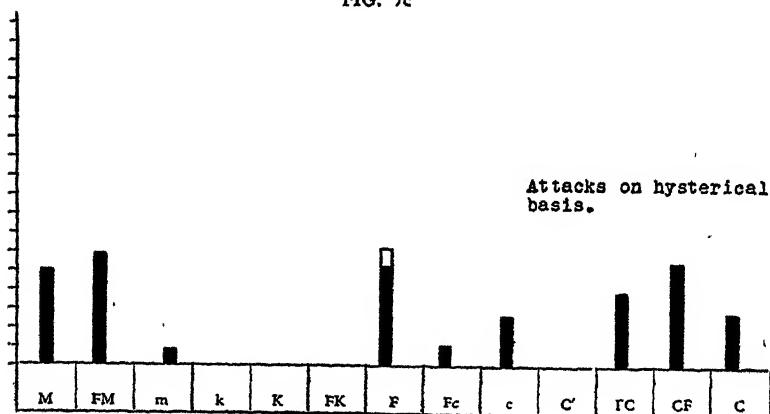


FIG. 5c



that a hysterical personality must be considered in all these cases a diagnosis which has been justified clinically?

Let us first consider the patient whose record is epitomised in figure 5b. This man is a soldier, aged 38, with a very limited intellectual capacity. The Rorschach record is obviously abnormal; to produce only three responses is in itself sufficient to show that there is a very serious psychological disturbance. But this alone would not rule out the possibility of a cerebral lesion, for we have seen in a few cerebral case records which are the results of such a restricted and reduced psychological capacity that only

three responses have been given. But the psychological ingredients in this case—5b—are different from those which remain in the case of a personality distorted through the effects of organic cerebral damage. This patient's psychological make-up, as exemplified in the Rorschach, shows explosive and primitive emotions to dominate, and to be unchecked by a single response indicating control. He must be considered therefore as a seriously disturbed personality in whom, at the time of examination, very primitive emotional factors are dominant, and who is devoid not only of the stabilizing influence of an adequate inner life, but even of the more ordinary standards of self-discipline. Despite the restricted output, therefore, the outstanding characteristic of this record is the undue strength of primitive emotional drives.

Figure 5a represents the record of a married woman of 50, coming from a good social background and of considerably better intellectual capacity than the patient previously discussed. Her record is not lacking in the element of self-control which was so striking a factor in the previous case—in fact the F percentage is high. However, she is unquestionably immature both in regard to her relations with other persons (only CF responses) and in regard to her own inner life, (FM far exceeding M). This immature picture at her age, and in view of her life-situation and opportunities, is certainly not normal. It shows a fundamentally unsatisfied person in great need of constant attention (*). Such a person might well find in the physical symptoms from which she was suffering a sure way to secure this attention.

Case 5c is perhaps the most interesting; it is the record of an extremely intelligent girl of 21, whose focal epileptic attacks were so perfect a reproduction of the genuine article as to have misled specialists for four years. The abnormality in this record lies in the fact that despite an excellent showing on 9 of the ten cards, despite evidence of rich psychological potentialities, she became profoundly disturbed when presented with the card designed by Rorschach to elicit sexual ideas. In view of this discrepancy in performance one had to question the possibility of a serious sexual

* The aspects of the record which would lead to this particular conclusion have not been discussed in this paper.

traumatic experience, a disturbance sufficiently deep-seated and profound so as to render her incapable of reply, and to break down in confusion, when presented with what was, to all intents and purposes, just another inkblot. Psychiatric investigation revealed that she had been raped by her uncle in early childhood, and the report of Dr. A. W. Young concludes with the statement: "Having repressed, as she has, her sexual experiences in childhood, a conflict has been produced and the emotion attached to these events has been expressed in motor symptoms."

Differentiating between the patients whose records are epitomized in figures 4a, b, and c, and those in figures 5a, b, and c, we may say, that whereas the former are depleted personalities—inadequate, negative—the latter have some positive difficulty related to the misdirection of psychological energies. Our work at the present time is concerned with this problem.

It should not be assumed from the foregoing discussion that a Rorschach diagnosis is a straight-forward, all or none, affair. For while many patients present personalities which allow of a clear-cut diagnosis with reference to their physical condition, this is by no means always the case.

There are, for example, some patients who present signs of an organic cerebral disturbance and at the same time some neurotic features, which are not only present in addition to the organic indications, but may actually mask or distort them. There are also borderline cases in regard to neurotic personalities which are hard to evaluate simply because any large sampling of the normal population will show that persons with as many as 4 or 5 neurotic signs in their Rorschach records may be carrying on in their jobs and living a seemingly normal life. There are schizoid personalities who are in one sense abnormal, but whose abnormality may not actually be an important factor in their present illness.

There is also much more to a good Rorschach interpretation than the mere classification of a patient into this or that clinical category. Even with similar clinical diagnoses, and sufficiently similar Rorschach records to allow of a diagnosis in terms of the personality structure, the records of any two patients will probably differ sufficiently to be worth an extensive report.

It is all too easy for the psychologist in clinical medicine to be concerned primarily with whether his diagnosis, made under the conditions of "blind analysis", *i.e.*, without knowledge of the clinical history, is correct,⁶ what is probably more important, however, for both the psychologist and clinician to recognize, is that diagnosis of a physical condition *per se* is not the function of the Rorschach examination or examiner. Such diagnoses have had to be attempted, for it was the natural method of demonstrating (the by no means universally accepted belief) that the patient's personality might be directly related to his physical condition. But in all probability the most valuable type of co-operation will be achieved when the physician has become convinced of the importance of this interrelation between psyche and soma, and through some contact with the Rorschach method has become aware of the varieties of personality ingredients which can, so to speak, be spread out for his inspection. When this is achieved the physician will probably wish to draw his own conclusions as to what a particular psychological equipment, adjustment or lack of it may mean in a given case; this would put the psychologist temporarily out of a job, but it would be worth it!

BIBLIOGRAPHY

1. HARROWER-ERICKSON, M. R. Personality studies in patients with cerebral lesions. Bulletin of the Canadian Psychological Association, pp. 9-10, December 1940.
2. HARROWER-ERICKSON, M. R. Personality changes accompanying cerebral lesions. I. Rorschach studies of patients with cerebral tumors. Archives of Neurology and Psychiatry, vol. 43, pp. 859-890, May 1940.
3. MIALE, F. R., and HARROWER-ERICKSON, M. R. Personality structure in the Psychoneurosis. Rorschach Research Exchange, vol. 4, No. 2, pp. 71-74, April 1940.
4. HARROWER-ERICKSON, M. R. Personality changes accompanying cerebral lesions. II. Rorschach studies of patients with focal epilepsy. Archives of Neurology and Psychiatry, vol. 43, pp. 1081-1107, June 1940.
5. HARROWER-ERICKSON, M. R. Psychological studies of patients with epileptic seizures. Chapter XX. in Epilepsy and Cerebral Localization by Penfield and Erickson, New York, Thomas, 1941.
6. ROSS, W. DONALD. The Contribution of the Rorschach Method to Clinical Diagnosis. Journal of Mental Science, July 1941.
7. KLOPFER, B. and KELLEY, D. M. The Rorschach Technique. World Book Co., New York.

PART VIII

CHILD PSYCHOLOGY

TRENDS IN CHILD PSYCHOLOGY *

HORACE B. ENGLISH

Ohio State University

A bird's eye view of the progress of a science, or of some part of it, seems comparatively easy until one is required to correlate it with the worm's-eye view. Then details previously overlooked cast doubt upon the broader generalizations. Yet both the generalizations and the correlation are needed.

Specific dates are among the details which obscure trends. Let us refuse here to be too precise. About two decades or so ago the interest in child psychology was systematic-genetic. It was hoped, that is, to find in the behavior of children the general principles needed to explain adult behavior. The function of speech in adults, for example, could only be understood in the light of its development in infants and children. One uses the plural here deliberately; it was the study not of the individual infant or child but of the general development of infants and children which was the focus of attention. Although most textbooks were written for prospective teachers, the categories under which the facts were listed were the categories of adult systematic psychology, not those of the problems which present themselves in school or home. Child psychology was supposed to help the teacher only in a general way—and did so. Experimental data were rather meager except in a few restricted fields.

Meanwhile, a number of things were happening which were to influence the subject. The testing of intelligence was providing

* A revision of an article originally published in the EDUCATIONAL RESEARCH BULLETIN, Vol. 15, 1936.

new insights into child behavior. Not, one hastens to add, because all or most problems are intellectual. Part of the contribution of the intelligence test lies in the evidence it yields that in a given individual case the problem is not a matter of intelligence. And the proof that enormous individual differences in intelligence were being obscured and even brought to naught by a routinized mass instruction brought with it a renewed emphasis upon individualized treatment of children.

The psychoanalytic movement also played its part in sharpening interest in individual children. Above all, however, the ensuing years were those in which the mental-hygiene point of view dominated child psychology. We began to count the cost of crime and delinquency, with their roots in the maladjustments of childhood and early adolescence. We became aware of the ravages of mental disorder and to suspect that these might be more largely prevented by early action. We began to hope that men and women might live happier and more wholesome lives if more attention were given to wholesome childhood. School psychologists hired primarily to give mental tests began to supplement these with more and more elaborate case studies and with the endeavor to help children in their emotional maladjustments. Child-guidance clinics were being founded in our chief cities. In 1919 there were five child-guidance units in the United States. In 1929 there were 67. The peak was reached in 1931 with 83, but despite the depression some 78 were still in operation in 1933.¹

The incorporation of mental-hygiene material and the emphasis upon the needs of the individual are well exemplified in J. J. B. Morgan's *The Psychology of the Unadjusted School Child* as well as in his *Child Psychology*, first issued in 1931, although the latter book overlaps with the later trends to be presently considered. As far as the school was concerned the mental-health point of view was greatly needed and has had, in the main, thoroughly beneficial results.

It is harder to estimate the effects on parents. One hears wild tales of the parent who has been rendered overapprehensive by

¹ Figures supplied from a graph in G. S. Stephenson, *Child Guidance Clinics*, p. 4.

reading the latest book on "Phobias in Infancy, Their Cause and Consequences." Personally, I have never met such extreme cases. For some obscure reason the person who takes the job of being a parent seriously has always been the butt of good-natured ridicule. Most of the jokes about parental reading in child psychology are varieties of this hardy perennial. Certainly, much is now known about the kinds of parental behavior which hinder or prevent wholesome childhood; it is difficult to see how a sensible dissemination of this information can be anything but beneficial.

On the other hand, it is the peculiar vice of the pathologist to lose perspective and to see abnormality everywhere. Mental hygienists are by no means free of this vice. They have so impressed us with the usefulness of the concepts derived from problem cases that the quip has gained currency that all children are problem children. In so far as this tends to "normalize" us in our attitude to peculiar children, it is good; but it is to be feared that it has the converse effect of setting us to hunt for peculiarities in perfectly "normal" children. For this reason, many of the books on mental hygiene for children are rather "bad medicine" for the uninstructed and uncritical reader. Fortunately there are many sane and helpful books.

Meanwhile a new tendency has begun to have its effect. Child psychology is at last becoming thoroughly experimental. A variety of influences has been at work to produce this result. The view put forward so polemically by J. B. Watson that man's native response equipment was very slender and that everything was the result of conditioning must have part of the credit. For, though he did rear a tremendous edifice of theory upon a slender foundation of fact, Watson did not limit himself to dogma. By experiment he found in infants no specific responses which could possibly be called fear to a wide variety of "fearful" objects, and he demonstrated the possibility of "conditioning" a fear to an originally neutral object. H. E. Jones, C. W. Valentine, and the present writer have pointed out that these results of Watson's have been over-interpreted. The important thing is that they started people to studying the emotional responses of children under carefully controlled conditions, both in the laboratory and

in the natural habitat. And once started, experimental study spread from emotions to the entire repertoire of human response.

These experimental studies cover the entire range of childhood. Under the direction of the late A. P. Weiss, Ohio State University took the leadership in the study of newly born infants. This work was primarily an extension of the earlier systematic-genetic approach and is, for the present at least, of little direct value to the educator or the child-development specialist. But experimental work was not limited to the neonate. New techniques for the study of children were devised—especially nursery school children. Experiment came out of the laboratory to study the child's integrated behavior in the home, on the playground, in the school. Activity check lists to render the observation more accurate and objective were planned. The motion-picture camera was early utilized.

Test techniques have been enormously enriched and expanded. The interest is no longer in test results and norms alone; rather, tests are utilized to discover what changes if any are being induced by certain environmental conditions. The old questionnaire has been overhauled until it is hardly recognizable. Indeed, so far has the improvement gone that there is grave danger that some will forget the inherent and ineradicable limitations of the instrument. The attitudes of children like those of adults have come under experimental scrutiny. The monumental work of the Character Education Inquiry and of its successors has shown that the growth of conduct, morality, and character can also be made to yield to experiment. Here again, in the writer's opinion, the data have been currently over-interpreted; but the enduring fact is that experiment has been set going. Further data will inevitably correct misinterpretations.

It has always been recognized that behavior may have a hidden significance. Now "projective" techniques have been designed and named which will elicit the sorts of behavior most likely to have such deeper meaning. In the case of the Rorschach Ink Blots, enormous attention has been given to the problem of quantitative scoring of replies, but the scores have been interpreted in terms of alleged "traits" which have received little critical at-

tention. Play with household toys and dolls has been used not only for the diagnosis of personality formations but for therapy as well—the child is encouraged to express his conflicts in permissive or accepting atmosphere.

The social environment of the child has received increasing attention. Lewin and his pupils have shown how an hypothesis or theory may lead to active experimenting and practical results. The concepts of a dynamic field, and of a life-space with boundaries of varying degrees of penetrability may sound rather formidable. But they have suggested ingenious and fruitful experiments upon the emotional climate in which the child lives. Less theoretically oriented is the attempt of the sociometrician to depict the varied personal relations which give structure to the child's social life.

Finally to be noted is the renewed interest in the child as a whole which finds expression in an emphasis upon child development rather than merely child psychology. Institutes of child development are springing up, to replace, in large part, the child-guidance clinics. In these institutes as in the clinics, problem children and the maladjusted are studied and given help, but normal children also are found worthy of intensive consideration.

And that consideration, though it utilizes case work and the older intuitive, clinical approach, also utilizes all the empirical and quantitative methods sketched above. The distinctive contribution of the trend toward child development is the insistence upon the correlation of every sort of information about the child. Physical growth must be considered along with development in intelligence, the child's class status and his various roles in child society must be considered along with his schooling, his physiological tensions along with his conflicts and frustrations. Longitudinal or ontogenetic studies of individuals take the center of the stage and cross-sectional studies of group tendencies are reduced to ancillary function. The whole child as a growing, learning organism in a physical and social environment is caught in focus.

The old interest in the child as father to the man is unlikely ever to die out. There will continue to be demands that child

psychology throw light upon the general problems of the science. The mental-hygiene interest in child psychology is also certain to be permanent. The case-study approach, if only for its reminder that we have to deal with a total individual and not with stimuli and responses only, will certainly continue to have its place in the textbook. But with the advent of active and intensive experimentation, child psychology has turned a corner. If it has not come of age, it may at least be said to have entered its adolescence. The next decade, one may predict, will see experiment richly extended to every aspect of child behavior. No generalization will be considered sound until it has been put to the test of experiment. Interest will be in the child's development for its own sake. The problems of the child himself will be dominant. Revolutionary changes of doctrines are probably not impending but we may confidently look forward to a steady advance in scientific knowledge and practical application.

I N D E X

- abnormal psychology, Part VI, 495-604
acoustic characteristics of the ear, 371-386
aesthetic experience, psychology of, Part V, 456-492
aesthetic experiences in current history, 457-467
affective character of music, 484-492; see 468-483
aggressive behavior, 200-230
American caste system and Negro intelligence, 607-620
"armchair psychology," 1-10
armchair taboo and progress of psychology, 1-10
animal psychology, Part III, 243-316
animal psychology, contemporary American, 306-316
ant learning, 276-305
anxiety hysteria, treatment of a case of, 534-546
Army personnel system, 635-651
aspiration, levels of, 319-345
audition, 371 ff.
authoritarian character structure, 231-242
authoritarian control, 202 ff.
- behavior, experimental study of mob, 153-174
books, the 100 greatest, 182-191
- child psychology, Part VIII, 705-710
choice, example of a method of social, 182 ff.
comparative psychology, ant learning as a problem in, 276-305
concept of social status, 128-145
conceptual focus of some systems of psychology, 49-63
- differential psychoogy, Part VII, 605-701
democratic control, 202 ff.
democratic leadership, training in, 175-181
- embryology of mind, experimental, 245-275
error, study of the perpetuation of, 192-199
experimental psychology, Part IV, 317-454
expression of meaning and emotion in music, 468-483
- fetal behavior, animal, 245 ff.
- Gestalt, 5 f.

- homeostasis, 24, 407 ff.
- hypnosis, 534 ff., 547 ff., 591
- identification and post-war world, 119-127
- identification with classes, 146-152
- insulin shock therapy, 518 ff.
- intelligence and mental disorder, 621-633
- intelligence of Negroes and the American caste system, 607-620
- laissez-faire control, 202 ff.
- leadership, training in democratic, 175 ff.
- leadership, types of, 202 ff.
- learning, ant, 276 ff.
- mental hygiene, Rorschach method and, 652-684
- metrazol shock therapy, 518 ff.
- mind-body problem, difficulties with, 64-93
- mob behavior, 153 ff.
- motivation, theory of, 22-48
- multiple personality, 591-604
- music, affective character of, 484-492; also 468 ff.
- ocular photography, 346-370
- operationism and relativism in psychology, 78 ff.
- operationism and logical positivism, 88 ff.
- personality and typology, 94-115
- personnel classification in the Army, 536-651
- pharmacological shock, 517-533
- physiological conquest of personality structure, 405-428
- psychological laws, 65 f.
- psychopathic personality, 507-515
- psychophysiology of set, 387-404
- Rorschach methodology, 652 ff., 685 ff.
- shock therapy, 517 ff.
- "social climates," 201 ff.
- social psychology, Part II, 117-242
- social status, concept of, 128-145
- social status scales, 139 f.
- types, pseudo- , 10-20
- typology, personality and, 94 ff.

